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Agricultural Research Institute

PUSA

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Preſent Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Conſiderable Parts of the WORLD.

VOL. LXIII. PART I.

LONDON:

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M.DCC.LXXIII.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume. And this information was thought the more necessary, not only as it had been the common opinion, that they were published by the authority, and under the direction, of the Society itself; but also, because several authors, both at home and abroad, have in their writings called them the *Transactions of the Royal Society*. Whereas in truth the Society, as a body, never did interest themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviseable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such, as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the

reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

At a COUNCIL, January 28, 1773.

Reſolved, That after Volume LXII. the *Philoſophical Tranſactions* be publiſhed twice in a year; the firſt publication to be of the months of November and December of the preceding year, and January and February of the current year, as ſoon as may be after February, under the name of the "firſt part" of the volume: and the ſecond publication to be of the remaining months unto the receſs of the Society, as ſoon as may be after the receſs, under the name of the "ſecond part" of the volume.

CON-

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VOL. LXIII. PART I.

V. *A*

XX. *A*

PHILOSOPHICAL TRANSACTIONS.

I. *An Account of the Discovery of the Manner of making Isinglass in* Russia; *with a particular Description of its Manufacture in* England, *from the Produce of* British *Fisheries. In a Letter from* Humphrey Jackson, *Esq; F. R. S. to* William Watson, *M. D. F. R. S.*

SIR,

Read Nov. 19, 1772.

YOUR distinguished zeal to promote science in general, and particularly such arts as tend to improve the commerce

 of

of your country, incites me to addreſs to you the following account of the genuine manufacture of foreign iſinglaſs, with the method of making it in England from Britiſh materials. If you ſhould judge the ſubject deſerving attention, you will do me great honour in preſenting this paper to the Royal Society: but this I beg leave to ſubmit to your judgement and candour, and to ſubſcribe myſelf,

With great eſteem,

S I R,

Your moſt obliged,

and moſt obedient

humble ſervant,

Great Tower-Hill,
June 15, 1772.

Humphrey Jackſon.

ALL authors, who have hitherto delivered processes for making icthyocolla, fish-glue or isinglass, have greatly mistaken both its constituent matter and preparation.

To prove this assertion, it may not be improper to recite what Pomet says upon the subject, as he appears to be the principal author whom the rest have copied *. After describing the fish, and referring to a cut engraved from an original in his custody, he says: 'As to the manner of making the 'isinglass, the sinewy parts of the fish are boiled 'in water, till all of them be dissolved that will dissolve; then the gluey liquor is strained, and set to 'cool. Being cold, the fat is carefully taken off, 'and the liquor itself boiled to a just consistency, 'then cut to pieces, and made into a twist, bent in 'form of a crescent, as commonly sold, then hung 'upon a string, and carefully dried.'

From this account, it might be rationally concluded that every species of fish which contained gelatinous principles would yield isinglass: and this parity of reasoning seems to have given rise to the hasty conclusions of those, who strenuously vouch for the extraction of isinglass from sturgeon; but as that fish is easily procureable, the negligence of ascertaining the fact by experiment seems inexcuseable.

Every traveller, as well as author, who mentions isinglass, observes that it is made from certain fish

* See Pomet's History of Drugs, and Caspar Neuman's Chemistry, English translations. Hist. Materiæ Medicæ, Vogel. Lewis's Materia Medica. Dossie's Institutes of Chemistry.

found in the Danube and rivers of Muſcovy. Willughby and others inform us, that it is made of the ſound of the * Beluga; Caſpar Newman that it is made of the Huſo Germanorum and other fiſh, which he has ſeen frequently ſold in the public markets of Vienna. Theſe circumſtances make it appear the more extraordinary, that a perfect account of the manufacture of ſuch an eſſential article of commerce ſhould remain ſo long unrevealed.

In my firſt attempts to diſcover the conſtituent parts and manufacture of iſinglaſs, relying too much upon the authority of ſome chemical authors, whoſe veracity I had experienced in many other inſtances, I found myſelf conſtantly diſappointed. Glue, not iſinglaſs, was the reſult of every proceſs; and although, in the ſame view, a journey to Ruſſia proved fruitleſs, yet a ſteady perſeverance in the reſearch proved not only ſucceſsful as to this object, but, in the purſuit to diſcover a reſinous matter plentifully procureable in the † Britiſh fiſheries, which has been found, by ample experience, to anſwer ſimilar purpoſes. It is now no longer a ſecret that our ‡ lakes and rivers in North America are ſtocked

* Vide Specimen Hiſtor. Nat. Volg. Auctore J. R. Forſter, Philoſ. Tranſ. 1767.

† Upwards of forty tons of Britiſh iſinglaſs have been manufactured and conſumed ſince this diſcovery was firſt made.

‡ As the lakes of North America lie nearly in the ſame latitude with the Caſpian Sea, particularly Lake Superior, which is ſaid to be of greater extent, it was conjectured they might abound with the ſame ſorts of fiſh, and, in conſequence of public advertiſements diſtributed in various parts of North America, offering premiums for the ſounds of ſturgeon, and other fiſh, for the purpoſe of making iſinglaſs, ſeveral ſpecimens of fine

with

with immenſe quantities of fiſh, ſaid to be the ſame ſpecies with thoſe in Muſcovy, and yielding the fineſt iſinglaſs, the fiſheries whereof, under due encouragement, would doubtleſs ſupply all Europe with this valuable article.

But to return, no artificial heat is neceſſary to the production of iſinglaſs, neither is the matter diſſolved for this purpoſe; for, as the continuity of its fibres would be deſtroyed by ſolution, the maſs would become brittle in drying, and ſnap ſhort aſunder, which is always the caſe with glue, but never with iſinglaſs. The latter, indeed, may be reſolved into glue with boiling water, but its fibrous recompoſition would be found impracticable afterwards, and a fibrous texture is one of the moſt diſtinguiſhing characteriſtics of genuine iſinglaſs. The reproduction of leather might, with equal reaſon, be attempted from the former.

A due conſideration that an imperfect ſolution of iſinglaſs, called fining by the brewers, poſſeſſed a peculiar property of clarifying malt liquors, induced me to attempt its analyſis in cold ſubacid menſtruums. One ounce and a half of good iſinglaſs, ſteeped a few days in one gallon of ſtale beer, was converted into good fining, of a remarkable thick conſiſtence: the ſame quantity of glue, under ſimilar treatment, yielded only a mucilaginous liquor, reſembling diluted gum-water, which, inſtead of clarifying beer, increaſed both its tenacity and turbidneſs, and communicated other properties in no

iſinglaſs, the produce of fiſh taken in theſe parts, have been lately ſent to England, with proper atteſtations as to the unlimited quantity which may be procured.

respect corresponding with those of genuine fining. On commixing three spoonfuls with a gallon of malt liquor, in a tall cylindrical glass, a vast number of curdly masses became presently formed, by the reciprocal attraction of the particles of isinglass and the feculencies of the beer, which, increasing in magnitude and specific gravity, arranged themselves accordingly, and fell in a combined state to the bottom, through the well-known laws of gravitation; for, in this case, there is no elective attraction, as some have imagined, which bears the least affinity with what frequently occurs in chemical decompositions.

These phænomena are adduced here as correlative proofs of the impracticability of making isinglass by the previous reduction of the finewy parts of fish into jelly; and it seems evident, that the clarifying action of isinglass depends principally upon a crude minute division, not solution of its parts, which is still farther confirmed, by diluting a few drops of fining with fair water in a glass; for thus the slender filaments become conspicuous to the eye, especially when assisted with a double convex lens, but these immediately disappear on an addition of hot water.

As the general processes for making isinglass appear from hence illusive and erroneous, the long concealed principles of its manufacture into the various common forms and shapes become more obvious and comprehensive. If what is commercially termed long or short stapled isinglass be steeped a few hours in fair cold water, the entwisted membranes will expand, and reassume their original beau-

ful * hue, and, by a dextrous addreſs, may be perfectly unfolded. By this ſimple operation, we find that iſinglaſs is actually nothing more than certain membranous parts of fiſhes, diveſted of their native mucoſity, rolled and twiſted into the forms abovementioned, and dried in the open air.

The ſounds, or air-bladders of freſh-water fiſh, in general, are preferred for this purpoſe, as being the moſt tranſparent, flexible, delicate ſubſtances. Theſe conſtitute the fineſt ſorts of iſinglaſs; thoſe called book and ordinary ſtaple, are made of the inteſtines, and probably the peritonæum, of the fiſh. The Beluga yields the greateſt quantity, as being the largeſt and moſt plentiful fiſh in the Muſcovy rivers; but the ſounds of all freſh-water fiſh yield, more or leſs, fine iſinglaſs, particularly the ſmaller ſorts, found in prodigious quantities in the Caſpian ſea, and ſeveral hundred miles beyond Aſtracan, in the Wolga, Yaik, Don, and even as far as Siberia, where it is called *kle* or *kla* by the natives, which implies a glutinous matter; it is the baſis of the Ruſſian glue, which is preferred to all other kinds for its ſtrength.

The anatomy and † uſes of the ſound in fiſh ſeems not yet adjuſted by ichthyologiſts. I have not met with a genuine deſcription of its ſituation and

* If the fine tranſparent iſinglaſs be held in certain poſitions to the light, it frequently exhibits beautiful priſmatic colours.

† Fiſhermen have a dextrous art in perforating the ſound of freſh-taken cod fiſh with a needle, in order to diſengage the incloſed air. Without this operation, the fiſh could not be kept under water in the well-boat, conſequently could not live; but if by accident the operator wounds an artery, the fiſh preſently dies, through the diſcharge of bood, to the loſs of the proprietor, who thus can ſeldom bring it ſweet to market.

figure

figure in any author. A modern writer * will have it to be the meſentery of the fiſh; but the celebrated Gouan, the lateſt, and perhaps the moſt accurate author on icthyology, gives a more ſatiſfactory and comprehenſive account of it, under the title of † *La Veſicule Aërienne.* Yet, if the identity of the air-bladder, and what, in Engliſh, is called ſound, be admitted, which ſeems particularly aſcertained in a certain genus, viz. the Aſellus of Willughby, or Gadus of Artedi, his deſcription is a little erroneous with reſpect to its termination near the *Veſica urinaria*; for in cod and ling, the continuation of the ſound, or air-bladder, may be eaſily traced from thence to the laſt *vertebra* adjoining the tail.

The ſounds, which yield the finer iſinglaſs, conſiſt of parallel fibres, and are eaſily rent longitudinally; but the ordinary ſorts are found compoſod of double membranes, whoſe fibres croſs each other obliquely, reſembling the coats of a bladder; hence the former are more readily pervaded and divided with ſubacid

* Doſſie, in Memoirs of Agriculture.

† La Véſicule aërienne eſt un ſac membraneux compoſé de deux ou trois envelopes, qui ſe ſeparent facilement, & rempli d'air, à la faveur duquel les poiſſons ſe ſoutiennent dans l'eau. Il eſt pour l'ordinaire ſitué en long, enfermé dans le peritoine, placé entre les vertebres & l'eſtomac. Sa longueur dépend de la capacité du bas ventre, & de la grandeur du poiſſon: il eſt tantôt cylindrique, elliptique, ové ou renverſé, tantôt à deux lobes & à deux loges, tantôt à trois lobes & à trois loges, &c. dans les males il deſcend preſque juſqu' à la region de la veſſie urinaire.

Cette Veſicule eſt attachée avec l'eſtomac, avec l'eſophage, ſans le diaphragme, tantôt par le côté tantôt par la pointe & ſ'y abbouche par un conduit pneumatique. Goüan, Hiſtoire des Poiſſons.

liquors;

liquors; but the latter, through a peculiar kind of interwoven texture, are with great difficulty torn aſunder, and long reſiſt the power of the ſame menſtruum; yet, when duly reſolved, are found to act with equal energy in clarifying liquors.

Iſinglaſs receives its different ſhapes in the following manner.

The parts, of which it is compoſed, particularly the ſounds, are taken from the fiſh while ſweet and freſh, ſlit open, waſhed from their ſlimy *ſordes*, diveſted of every thin membrane which invelopes the ſound, and then expoſed to ſtiffen a little in the air. In this ſtate, they are formed into rolls about the thickneſs of a finger, and in length according to the intended ſize of the ſtaple: a thin membrane is generally ſelected for the center of the roll, round which the reſt are folded alternately, and about half an inch of each extremity of the roll is turned inwards. The due dimenſions being thus obtained, the two ends of what is called ſhort ſtaple are pinned together with a ſmall wooden * peg; the middle of the roll is then preſſed a little downwards, which gives it the reſemblance of a heart ſhape, and thus it is laid on boards, or hung up in the air to dry. The ſounds, which compoſe the long-ſtaple, are larger than the former; but the operator lengthens this ſort at pleaſure, by interfolding the ends of one or more pieces of the ſound with each other. The extremities are faſtened with a peg, like the former; but the middle part of the roll is bent more conſiderably down-

* See the annexed Drawings [TAB. I. Fig. 1.]

wards; and, in order to preſerve the * ſhape of the three obtuſe angles thus formed, a piece of round ſtick, about a quarter of an inch diameter, is faſtened in each angle with ſmall wooden pegs, in the ſame manner as the ends. In this ſtate, it is permitted to dry long enough to retain its form, when the pegs and ſticks are taken out, and the drying completed; laſtly, the pieces of iſinglaſs are colligated in rows, by running packthread through the peg-holes, for convenience of package and exportation.

The membranes of the † book ſort, being thick and refractory, will not admit a ſimilar formation with the preceding: the pieces therefore, after their ſides are folded inwardly, are bent in the center, in ſuch manner that the oppoſite ſides reſemble the cover of a book, from whence its name; a peg being run acroſs the middle, faſtens the ſides together, and thus it is dried like the former. This ſort is interleaved, and the pegs run acroſs the ends, the better to prevent its unfolding.

That called cake iſinglaſs is formed of the bits and fragments of the ſtaple ſorts, put into a flat metalline pan, with a very little water, and heated juſt enough to make the parts cohere like a pancake, when it is dried; but frequently it is overheated, and ſuch pieces, as before obſerved, are uſeleſs in the buſineſs of fining. Experience has taught the conſumers to reject them.

Iſinglaſs is beſt made in the ſummer, as froſt gives it a diſagreeable colour, deprives it of weight, and

* See Fig. 3.
† Fig. 4.

impairs

impairs its gelatinous principles; its faſhionable forms are unneceſſary, and frequently injurious to its native qualities. It is common to find oily putrid matter and *exuviæ* of inſects between the implicated membranes, which, through the inattention of the cellarman, often contaminate wines and malt liquors in the act of clarification. Theſe peculiar ſhapes might, probably, be introduced originally with a view to conceal and diſguiſe the real ſubſtance of iſinglaſs, and preſerve the monopoly; but, as the maſk is now taken off, it cannot be doubted to anſwer every purpoſe more effectually in its * native ſtate, without any ſubſequent manufacture whatever, eſpecially to the principal conſumers, who hence will be enabled to procure ſufficient ſupply from the Britiſh colonies. Until this laudable end can be fully accompliſhed, and as a ſpecies of iſinglaſs, more eaſily producible from the marine fiſheries, may probably be more immediately encouraged, it may be manufactured as follows.

The ſounds of cod and ling bear great analogy with thoſe of the *accipenſer genus* of Linnæus and Artedi, and are in general ſo well known, as to require no particular deſcription. The Newfoundland and Iceland fiſhermen ſplit open the fiſh, as ſoon as taken, and throw the back-bones, with the ſounds annexed, in a heap; but, previous to incipient putrefaction, the ſounds are cut out, waſhed from their ſlimes, and ſalted for uſe. In cutting out the ſounds, the intercoſtal parts are left behind, which are much the beſt; the Iceland fiſhermen are ſo ſenſible of

* See Fig. 5.

this, that they beat the bone upon a block with a thick ſtick, till the pockets, as they term them, come out eaſily, and thus preſerve the ſound entire. If the ſounds have been cured with ſalt, that muſt be diſſolved by ſteeping them in water, before they are prepared for iſinglaſs; the freſh ſound muſt then be laid upon a block of wood, whoſe ſurface is a little-elliptical, to the end of which a ſmall hair bruſh is nailed, and with a * ſaw-knife, the membranes on each ſide of the ſound muſt be ſcraped off. The knife is rubbed upon the bruſh occaſionally, to clear its teeth; the pockets are cut open with ſciſſars, and perfectly cleanſed of the mucous matter with a coarſe cloth: the ſounds are afterwards waſhed a few minutes in lime-water, in order to abſorb their oily principle, and laſtly in clear water. They are then laid upon nets, to dry in the air; but, if intended to reſemble foreign iſinglaſs, the ſounds of cod will only admit of that called book, but thoſe of ling both ſhapes. The thicker the ſounds are, the better the iſinglaſs, colour excepted; but that is immaterial to the brewer, who is its chief conſumer.

This iſinglaſs reſolves into fining, like the other ſorts, in ſubacid liquors, as ſtale beer, cyder, old hock, &c. and in equal quantities produces ſimilar effects upon turbid liquors, except that it falls ſpeedier and cloſer to the bottom of the veſſel, as may be demonſtrated in tall cylindrical glaſſes; but foreign iſinglaſs retains the conſiſtency of fining preferably in warm weather, owing to the greater tenacity of its native mucilage.

* See Fig. 2.

Vegetable

Vegetable acids are, in every reſpect, beſt adapted to fining: the mineral acids are too corroſive, and even inſalubrious in common beverage.

It is remarkable that, during the converſion of iſinglaſs into fining, the acidity of the menſtruum ſeems greatly diminiſhed, at leaſt to taſte, not on account of any alkaline property in the iſinglaſs, probably, but by its inveloping the acid particles. It is likewiſe reducible into jelly with alkaline liquors, which indeed are ſolvents of all animal matters; even cold lime-water diſſolves it into a pulpous *magma*. Notwithſtanding this is inadmiſſible as fining, on account of the menſtruum, it produces an admirable effect in other reſpects: for, on commixture with compoſitions of plaſter, lime, &c. for ornamenting walls expoſed to viciſſitudes of weather, it adds firmneſs and permanency to the cement; and if common brick-mortar be worked up with this jelly, it ſoon becomes almoſt as hard as the brick itſelf: but, for this purpoſe, it is more commodiouſly prepared, by diſſolving it in cold water, acidulated with vitriolic acid; in which caſe, the acid quits the jelly, and forms with the lime a *ſelenitic* maſs, while, at the ſame time, the jelly being deprived, in ſome meaſure, of its moiſture, through the formation of an indiſſoluble concrete amongſt its parts, ſoon dries, and hardens into a firm body; whence its ſuperior ſtrength and durability are eaſily comprehended.

It has long been a prevalent opinion, that ſturgeon, on account of its cartilaginous nature, would yield great quantities of iſinglaſs; but, on examination, no part of this fiſh, except the inner coat of the ſound, promiſed the leaſt ſucceſs. This being full

of

of *rugæ*, adheres ſo firmly to the external membrane, which is uſeleſs, that the labour of ſeparating them ſuperſedes the advantage. The inteſtines, however, which in the larger fiſh extend ſeveral yards in length, being cleanſed from their mucus, and dried, were found ſurprizingly ſtrong and elaſtic, reſembling cords made with the inteſtines of other animals, commonly called cat-gut, and, from ſome trials, promiſed ſuperior advantages, when applied to mechanic operations.

Having now ſufficiently revealed the principal *arcana* in the manufacture of iſinglaſs, and explained ſome of its leaſt known phænomena and properties, the farther proſecution thereof, as a commercial buſineſs, is left to others, whoſe future inquiries into the ſubject, it is hoped, will, in ſome reſpect, be anticipated through this narrative; but whatever ſucceſs may attend the attempt, I flatter myſelf to ſtand acquitted, in having contributed every thing in my power to its advancement and perfection.

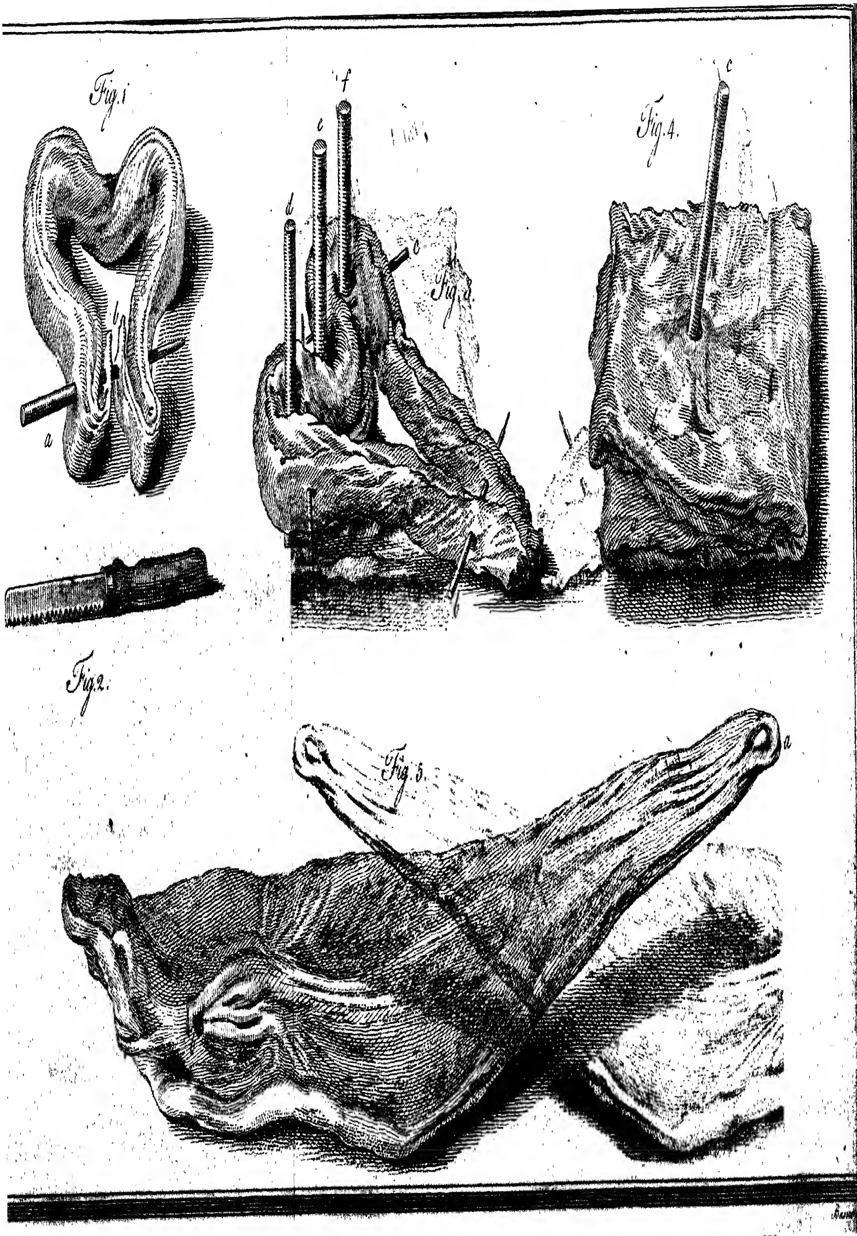


Fig. 1
a
b
Fig. 2
Fig. 3
c
d
e
f
Fig. 4
c
Fig. 5
a

EXPLANATION of the Figures; TAB. I.

Fig. 1. Short ſtaple iſinglaſs.
a The wooden peg, which faſtens the two ends of the iſinglaſs.
b The extremities folded inwards.

Fig. 2. The ſaw-knife.

Fig. 3. Long ſtaple iſinglaſs.
a b c Wooden pegs or pins.
d e f Three round ſticks, or pieces of wood, faſtened in the angles of the iſinglaſs by the pins *a* and *c*.

Fig. 4. Book iſinglaſs.
a b The two ſides, which reſemble the cover of a book.
c The wooden pin run through the ſides, to faſten them together.

Fig. 5. The entire ſound, or *veſica aeria*, of the iſinglaſs fiſh dried in its natural ſtate.
a The hole made for the packthread, wherewith it is hung up to dry.
b The orifice of part of the pneumatic veſſels left with the ſound.

II. *A Letter to* Charles Morton, *M. D. Sec. R. S. from Mr.* Adam Walker; *containing an Account of the Cavern of* Dunmore Park, *near* Kilkenny, *in* Ireland.

Dublin, April 26, 1771.

SIR,

Read Nov. 19, 1772.

AS I do not find in your Tranfactions any account of the cave of Dunmore Park, about three miles Weft of Kilkenny, I beg leave to lay before your learned Society an account of this fingular cavern, as near as an eye-furvey, and a few experiments on its ftones and petrefactions, will admit. It is fituated in a fine plain, rifing indeed here and there into fmall hills. The country all round abounds with limeftone, and quaries of beautiful black marble, variegated with white fhells. Different from thofe of Derbyfhire and Mendip, this cave defcends perpendicularly 30 yards, from the top of a fmall hill, through an opening 40 yards in diameter. The fides of this pit are limeftone-rock, whofe chinks nourifh variety of fhrubs and trees, down which the infpector muft defcend with great caution. In this defcent, he is amufed

amufed with flights of wild pigeons, and jackdaws from the cave below. When he reaches the bottom, he fees one fide of this pit fupported by a natural arch of rock, above 25 yards wide, under which he goes horizontally, and fees two fubterraneous openings to the right and left. If he turns to the right, he makes his way over rocks and ftones, coated with fpar in the moft whimfical fhapes, and formed from the dropping roof, juft as the dripping of a candle would cover a pebble. Thefe knobs take a fine polifh, are tranfparent, and variegated with the wildeft affemblage of colouring. The Earl of Wandesford had one of them fawn into a flab, and it is as beautiful as a Moco. When I tried thefe petrefactions with an acid, the effervefcence was exceffive ftrong; and, as the earth all round is calcareous, and the ftones limeftone, I humbly apprehend the icicle figures impending from the roof, and thefe knobs, are thus formed. The rains, that fall on the hill over this cavern, oozing through an okery calcareous earth, and the limeftone roof, imbibe or diffolve their fine particles in their defcent; and, as this mixture can only filter through the rock exceedingly flowly, the water hanging on the roof is foon diffolved by the air, and the ftony particles are left behind. Hence are formed the icicle-fhaped cones that hang from the roof; thefe growing perpetually longer, have, in many parts of the cave, met the knobs from the bottom, and formed a number of fantaftic appearances, like the pillars of a Gothic cathedral, organs, croffes, &c. When the rain filters pretty faft through the roof, it falls on the rocks below, and grows there into knobs and cones,

 whofe

whoſe vertex points to thoſe that impend from the roof.

A ſpectator, viewing theſe, cannot but conceive himſelf in the mouth of a huge wild beaſt, with ten thouſand teeth above his head, and as many under his feet. The ſcene is indeed both pleaſing and awe-ful; the candles burning dim, from the moiſture in the air, juſt ſerved to ſhew a ſpangled roof perpetually varniſhed with water, in ſome places upwards of 20 yards high; in other places we crawled on all-four, through cells that will but admit one at a time. After having ſcrambled about 500 yards into this (which I will beg leave to call the) right-hand part of the cave, we returned to day-light, and then proceeded to view the left-hand part. Here, as our guides informed us there were many different branches of the cavern, we tied one ball of pack-thread to another, as we went forward, that we might more eaſily find our way back. This branch is not ſo horizontal as the other; it inclines downwards, and the openings in it are vaſtly wider, ſome being at leaſt 100 yards wide, and above 50 high. A ſmall rill accompanied us, which, by its different falls, formed a ſort of rude harmony, well ſuited to the place. In a ſtanding part of this brook, and near a quarter of a mile from the entrance, we found the bones of a hundred at leaſt of the human race; ſome were very large, but when taken out of the water, they crumbled away. As we could find nothing like an inſcription, or earth for a burying-place, we conjectured that ſome of the civil wars, perhaps that of 1641, might have driven the owners of theſe bones into this place. The tradition of the neighbourhood threw no light upon it.

Many

Many of the rocks on the roof and ſides of this cavern are black marble, full of white ſpots of a ſhell-like figure; and the whole neighbourhood is full of quarries of this beautiful ſtone, which takes a fine poliſh, and is uſed through the three kingdoms for ſlabs, chimney-pieces, &c. I obſerved, in ſome deep and wet parts of theſe quarries, this elegant foſſil in the firſt ſtages of its formation; the ſhells are real, but ſo ſoftened by time and their moiſt ſituation, as to be ſuſceptible of receiving the ſtony particles into their pores, by whoſe coheſive quality, they in time become thoſe hard white curls that give value to the marble: and it is very remarkable, and a proof that theſe white ſpots have been real ſhells, and thus formed, that the longer a chimney-piece or ſlab is uſed, the more of thoſe ſpots ripen into view.

I have taken many more notes of the natural curioſities in this kingdom, which I ſhall be happy to communicate to your reſpectable Society, if you think the ſubjects of ſufficient importance; and am,

With great reſpect,

SIR,

Your moſt obedient,

humble ſervant,

Adam Walker.

III. *A ſhort Account of ſome Specimens of native Lead found in a Mine of* Monmouthſhire: *In a Letter from* Michael Morris, *M. D. F. R. S. to* M. Maty, *M. D. Sec. R. S.*

Fludyer's-Street, Weſtminſter, Nov. 5, 1772.

SIR,

Read Nov. 19, 1772.

ABOUT the middle of laſt July, I received three ſpecimens of lead-ore from Valentine Morris, Eſq; of Piercefield, in Monmouthſhire. They were dug up in one of his fields, on making ſome drains, at no conſiderable depth; they were marked N° 1, 2, 3. On reducing to powder an ounce and a half of the ore, marked N° 3, in order to aſſay it, I perceived that ſeveral ſmall bits were flatted by the peſtle, which, on a farther examination, proved to be native lead. Though the bits of lead are inconſiderable, yet, as they are the firſt that have been publickly ſeen in England, or, that I know of, in Europe, ſome of the beſt and lateſt writers on mineralogy declaring that they have not met with any, I thought it my duty to acquaint the Royal Society with the fact, that the firſt account

account of native lead may appear in the Philosophical Transactions, as well as the first account of native tin.

N. B. In more than 300 assays of lead ore, I have met with nothing of the kind before.

I am,

Yours. &c.

M. Morris.

IV. *Farther*

IV. *Farther Remarks upon a Denarius of the* Veturian *Family, with an* Etruſcan *Inſcription on the Reverſe, formerly conſidered. In a Letter to* Mathew Maty, *M. D. Sec. R. S. from the Reverend* John Swinton, *B. D. F. R. S. Cuſtos Archivorum of the Univerſity of* Oxford, *Member of the Academy* degli Apatiſti *at* Florence, *and of the* Etruſcan *Academy of* Cortona *in* Tuſcany.

Received June 20, 1772.

S I R,

I.

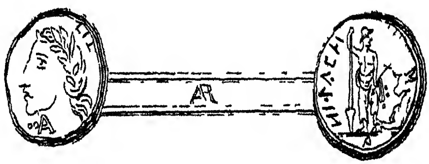
Read Nov. 19, 1772.

SOME years ſince (1), I offered my thoughts upon an inedited Samnite denarius, in my ſmall collection, with [See TAB. II. n. 1.] the Samnite-Etruſcan letters V˥V̇⅃·IИ, as I then apprehended, upon the reverſe. But as the two laſt letters were ill preſerved, or rather in part defaced, I was not intirely ſatisfied with my lection of the

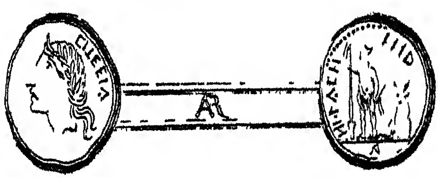
(1) *Philoſoph. Tranſact.* Vol. LVIII. p. 253—261.

inſcription

Tab.II. n. 1



Tab.II. n. 2.



inſcription to which they appertained. I have, however, ſince met with the [See TAB. II. n. 2.] ſame coin, finely preſerved, in the very valuable cabinet of the Reverend and learned Dr. Milles (*), Dean of Exeter, Preſident of the Society of Antiquaries, and Fellow of the Royal Society, with three letters, in the room of the two ſuppoſititious ones, upon it, perfectly formed; by the aſſiſtance of which, I have been enabled to give the true reading of the inſcription, and to arrive, I would flatter myſelf, at a full and complete interpretation of it. In order, therefore, to rectify all former miſtakes, ariſing from the injury received by the three laſt elements from time, I ſhall now beg leave to tranſmit this ſhort paper to you, to be laid before the Royal Society; not doubting but it will meet with the ſame candid acceptance, the ſame favourable reception, with which my other papers have been honoured, through the whole courſe of my correſpondence with you and two of your predeceſſors, from that very learned and moſt illuſtrious body.

(*) Towards the cloſe of May, 1771, the Dean ſent me a draught of the Samnite coin conſidered in this paper, very accurately taken; ſoon after which, the original inſcription on the reverſe of the medal, by an unlucky accident, was totally defaced. However, by the aſſiſtance of the draught ſent me, and my ſimilar coin, on which the remains of the three injured letters were rendered more viſible, by the help of a pretty good glaſs, and an attentive compariſon of them with the correſpondent elements on the draught the Dean communicated to me, the inſcription exhibited by my medal, as well as the other given here, approaches the truth as near as poſſible.

II.

I am then fully convinced from the Samnite, or Samnite-Etruſcan, inſcription formerly viſible on the reverſe of the Dean's very valuable denarius, which I ſhall beg leave to conſider here, that the true legend exhibited by my coin is ᗡꟽ⊦I⊐V˩·IИ, NI. LVFII, or LVVII, MER, equivalent to NI. LVFIVS, or LVVIVS, MERRISS, MERRIX, or MEDDIX. I ſay equivalent to LVFIVS, or LVVIVS, becauſe the Etruſcan, or Samnite-Etruſcan, letter ⊐ is endued with the power of V (2), as well as that of F, even (3) on this ſpecies of Samnite coins; and the Samnite termination II ſeems ſometimes at leaſt to have anſwered to the Roman, or Latin, IVS, as has been formerly obſerved. Theſe points are clearly evinced by the authors here referred to, as well as others, that might, with equal facility, be produced. As for the elements ᗡꟽ, they very probably repreſented the ſyllable MER; nothing being more common than the ſuppreſſion of a vowel between two conſonants, in antient Etruſcan words, as I have elſewhere demonſtratively proved (4). To the inſtances there mentioned I might add many

(2) Anton. Franciſc. Gor. *Muſ. Etruſc.* Vol. II. p. 414, 415. *Philoſoph. Tranſact.* Vol. LII. Par. I. p. 28—39.

(3) *Philoſoph. Tranſact.* ubi ſup. & Vol. LI. Par. II. p. 853—865. Anton. Franciſc. Gor. ubi ſup. & alib. Sig. Olivier. in *Sag. di Diſſertaz. di Corton.* Tom. II. p. 49—73. & Tom. IV. p. 133—149. *Philoſoph. Tranſact.* Vol. LII. Par. I. p. 34, 35. Lond. 1762.

(4) *Philoſoph. Tranſact.* Vol. LXI. p. 85, 86, 87.

others,

others, were it in any manner neceſſary. But as this is not the caſe, it will be ſufficient to reſer to the paper (5) wherein the former inſtances are contained.

III.

That the Samnite letters ꓤꟽ ſtood (*) for the word MERRISS, or MERRIX, uſed at Herculaneum, and the neighbouring part of Campania, for MEDDIX, denoting the chief magiſtrate of the Oſcans and Samnites, as we learn from Ennius (6) and Feſtus (7), will not be denied, as I apprehend, by

(5) *Philoſoph. Tranſact.* ubi ſup.

(*) The Samnites and Etruſcans by a ſingle ſyllable not ſeldom expreſſed a whole word on their coins. Thus for AИV⅃, LVNA, we find ИV⅃, LVN, or V⅃, LV, on the pieces ſtruck in the city of LVNA; VT, TV, for TVTERE, on thoſe of TVDER, or TODI; A⅃T, TLA, for TELAMON, on the medals attributed to the city going under that name, at preſent called TELAMONE; IAꟻ, FAI, on thoſe of FÆSVLÆ, now denominated FIESOLE, to omit many other ſimilar inſtances that occur. And as the Romans on their conſular coins exhibited COS for CONSVL, DICT for DICTATOR, &c. the Samnites and Oſcans on their medals probably uſed ꓤꟽ, MR, or MER, for MERRISS, or MEDDIX, the ſupreme magiſtrate, or rather one of the two ſupreme magiſtrates, of the city, or country, where the pieces I am conſidering were ſtruck. Hence it ſeems to appear, that the Romans borrowed the ſhort manner of writing here mentioned either of the Samnites, the Oſcans, or the proper Etruſcans. *Sag. Diſſertaz. di Corton.* Tom. II. p. 53. 41. *Philoſ. Tranſact.* Vol. LIV. p. 100. 103. *Una Lettera Al Sig. Abat. Barthelemy di* Annibale degli Abati Oliveri, &c. p. 43. In Peſaro, 1757.

(6) Ennii fragm. in *Annal.* viii. Vid. etiam not. Franciſc. Heſſelii in loc. p. 147, 148. Amſtelodami, 1707.

(7) Feſt. in voc. *Meddix.*

 any

any one who has read what has been advanced on this head by Sig. Avvocato (8) Paſſeri. This magiſtrate is called (*) Meddixtuticus by (9) Livy. NI. LVFIUS, or LVVIVS, therefore, ſeems not to have been one of the Italian generals in the Social War, as (10) I formerly ſuppoſed, but one of the chief magiſtrates either of the Oſcans or the Samnites, coeval with that war; there having (11) been two ſuch magiſtrates, anſwering to the two Roman Conſuls, and the two Carthaginian Suffetes, in both thoſe nations. That D inverted, ɑ, amongſt the antient Etruſcans, and the Samnites, had the power of R, whence it came to paſs that theſe two elements were looked upon as nearly related to each other, and R not infrequently uſed for D, we learn from the (12) famous Matthæus Ægyptius and Father (13) Gori. We are not therefore to be

(8) Joan. Baptiſt. Paſſer. Piſaurenſ. *Junonal. Sacr. Menſ. Herculanenſ.* in *Symbol. Litterar.* &c. Vol. I. p. 209, 210. Florentiæ, 1748.

(*) The Meddixtuticus, or Mediaſtuticus, of Livy ſeems to be formed of the two Samnite-Etruſcan words ƧƧIɑɑƎM, or Ƨꓘ⊦ɑɑƎM, Ƨꓘ⊦TꟻVT, MERRISS, MERRICKS, or MEDDIX, TVF TICKS, or TVTICKS, not TVCTICKS, as Sig. Paſſeri has written and pronounced it; both which words ſeem to denote the ſupreme magiſtrate, or rather one of the two ſupreme magiſtrates, amongſt the Oſcans and the Samnites. Paſſer. ubi ſup. p. 210—213.

(9) Liv. Lib. xxvi. c. 6.

(10) *Philoſoph. Tranſact.* Vol. LVIII. p. 258.

(11) Jo. Bapt. Paſſer. Piſaurenſ. ubi ſup.

(12) Matthæus Ægypt. in *Explicat. S. C. Romanor. de Bacchanal.* p. 148, 149. Neapoli, 1729.

(13) Anton. Franciſc. Gor. *Muſ. Etruſc.* Vol. II. p. 412, 413. Florentiæ, 1737.

ſurprized

ſurprized that MERRISS, MERRIX, and MEDDIX, ſhould be conſidered as the ſame word, or at leaſt as words of the ſame ſignification, by the celebrated Sig. (14) Giovanni Battiſta Paſſeri; who has ſet the point I am now upon, as well as every thing he has advanced relative to it, in the ſtrongeſt and cleareſt light.

IV.

That the Luvian, or Luſian, family was ſettled in that part of Campania poſſeſſed by the Oſcans and Samnites, or at leaſt almoſt contiguous to it, may be deemed highly probable, from the following inſcription (15), found near Venafro, the antient Venatrum, at no very great diſtance from Herculaneum, where the word MERRISS, or MERRIX, for MEDDIX, was antiently uſed, and now to be ſeen near that city (16).

LVVIA M. F. POSTVMA
SIGNVM. ET BASIM. D.
IVNONI. REG. SACRVM.

Whence, in conjunction with what has been already obſerved, we may conclude, that the coins I have been offering my thoughts upon may be preſumed to have made their firſt appearance amongſt either the Samnites or the Oſcans, probably the former, ſettled in a part of Campania, at no very great diſtance from Herculaneum; and that Ni. Luvius,

(14) J. Bapt. Paſſer. Piſaurenſ. ubi ſup.
(15) Ludovic. Anton. Murator. *Theſaur. veter. inſcript.* &c. Tom. I. P. xvi. n. 4. Mediolani, 1739.
(16) Idem ibid.

vius, or Lufius, was the Merrifs, Merrix, or Meddix, or at leaft one of the two magiftrates going under that denomination, of the city, if not the whole country, where it was ftruck.

V.

With regard to the year, as well as the place, wherein the medals in view firft appeared, neither the one nor the other can, with precifion, be afcertained; though the Latin A on one fide of my denarius and the Etrufcan A on the reverfe, in the exergue, feem to point at the initial letter of the name of the place where they were ftruck. And that this operation happened about the time of the Social War, we may pronounce highly probable, from the agreement of thefe denarii in moft particulars with other Samnite coins, undoubtedly ftruck not long after (*) the commencement of that war.

VI.

It would be wholly unneceffary, and altogether immaterial, to mention the different explications of the fymbol on the reverfes of thefe medals, offered by feveral learned men, here; as fcarce any of them feems to be perfectly agreeable to truth, or to be fuch as may be abfolutely depended upon. I fhall therefore content myfelf with obferving, that fome light is thrown upon this intricate fubject

(*) This feems likewife, from what has been already obferved in a former paper, pretty clearly to appear. *Philofoph. Tranfact.* Vol. LVIII. p. 256, 257.

by

by a curious passage in (17) Strabo; that the two writers who have handled this matter in the most copious, and learned, manner are Sig. (18) Bianconi and (19) M. Pellerin; that the medals I have been considering have notified to us the civil office, or dignity, of Ni. Luvius, or Lusius, as some other similar Samnite denarii have done the military post of (20) Papius Mutilus; and that the name of the Lusian family has never yet appeared, as I apprehend, on any other ancient coins. I shall only beg leave to add, that I remain, with all possible consideration and esteem,

SIR,

Your most obliged,

and most obedient,

humble servant,

Christ-Church, Oxon.
June 18, 1772.

John Swinton.

(17) Strab. *Geogr.* Lib. v. p. 250. Lutetiæ Parisiorum, 1620.
(18) Joan. Bapt. Biancon. *de Prisc. Hebræor. et Græcor. Lit. Libel.* p. 73. Bononiæ, 1748.
(19) *Supplem. aux Recueils des Medaill.* p. 11, 12. A Paris, 1765.
(20) *Philosoph. Transact.* Vol. LI. Par. II. p. 857. Vol. LII. Par. I. p. 28—39. & Vol. LIX. p. 432—444. Lond. 1769.

V. *A Catalogue of the Fifty Plants, from* Chelſea *Garden, preſented to the* Royal Society, *by the Worſhipful Company of Apothecaries for the Year* 1771, *purſuant to the Direction of the late Sir* Hans Sloane, *Bart. M. D. Soc. Reg. nuper Præſes: By* Staneſby Alchorne, *Member of the ſaid Society of Apothecaries in* London.

Read Nov. 26, 1772.

2451 ACER, *Pſeudo-Platanus*, foliis quinque lobis inæqualiter ſerratis, floribus racemoſis. Lin. Spec. plant. 1495. 2.
Acer montanum candidum. Bauh. pin. 430.

2452 Æſculus, *Hippo-Caſtanum*, floribus heptandris. Lin. Spec. plant. 488. 1.
Caſtanea folio multifido. Bauh. pin. 419.

2453 Andromeda, *Polifolia*, pedunculis aggregatis, corollis ovatis, foliis alternis lanceolatis revolutis. Lin. Spec. plant. 564. 5.
Erica humilis roſmarini foliis, unedonis flore, capſula ciſtoide. Pluck. alm. tab. 175. fig. 3.

2454 Aquilegia, *vulgaris*, nectariis incurvis. Lin. Spec. plant. 752. 1. α.

Aquilegia

Aquilegia ſylveſtris. Bauh. pin. 144.

2455 Arbutus, *Andrachne*, caule arboreo, foliis glabris integerrimis, baccis polyſpermis. Lin. Spec. plant. 566. 2.

Arbutus folio non ſerrato. Bauh. pin. 460.

2456 Artemiſia, *Dracunculus*, foliis lanceolatis glabris integerrimis. Lin. Sp. plant. 1189. 19.

Dracunculus hortenſis. Bauh. pin. 98.

2457 Artemeſia, *vulgaris*, foliis pinnatifidis planis inciſis; ſubtus tomentoſis; racemis ſimplicibus, floribus ovatis, radio quinquefloro. Lin. Spec. plant. 1189. 16.

Artemiſia vulgaris major. Bauh. pin. 137.

2458 Betula, *nana*, foliis orbiculatis crenatis. Lin. Spec. plant. 1394. 4. Flor. Lap. tab. 6. fig. 4.

Betula pumila. Laeſ. Pruſſ. 10.

2459 Caltha, *paluſtris*. Lin. Spec. plant. 784. 1. *α*.

Caltha paluſtris flore ſimplici. Bauh. pin. 276.

2460 Capſicum, *annuum*, caule herbaceo, pedunculis ſolitariis. Lin. Spec. plant. 270. 1.

Piper Indicum vulgatiſſimum. Bauh. pin. 102.

2461 Capſicum, *fruteſcens*, caule fruticoſo, pedunculis geminis. Lin. Spec. plant. 271. 2. *β*.

Capſicum minus, fructu parvo pyramidali erecto. Sloan. Hiſt. I. 240. tab. 146. fig. 2.

2462 Centaurea, *benedicta*, calycibus duplicato-ſpinoſis lanatis involucratis, foliis ſemidecurrentibus

rentibus denticulato-ſpinoſis. Lin. Spec. plant. 1296. 42.

Carduus ſylveſtris hirſutus, ſ. Carduus benedictus. Bauh. pin. 378.

2463 Cupreſſus, *ſempervirens*, foliis imbricatis, frondibus quadrangulis. Lin. Spec. plant. 1422. 1. *α*.

Cupreſſus. Bauh. pin. 488.

2464 Daphne, *Laureola*, racemis axillaribus, foliis lanceolatis glabris. Lin. Spec. plant. 510. 6.

Laureola ſempervirens flore viridi, quibuſdam Laureola mas. Bauh. pin. 662.

2465 Daphne, *Mezereum*, floribus ſeſſilibus ternis caulinis, foliis lanceolatis deciduis. Lin. Spec. plant. 509. 1.

Laureola folio deciduo, flore purpureo, officinis Laureola fœmina. Bauh. pin. 462.

2466 Dirca, *paluſtris*. Lin. Spec. plant. 512. 1.

Thymelæa floribus albis primo vere erumpentibus, foliis oblongis acuminatis, viminibus & cortice valde tenacibus. Geor. Virg. 155.

2467 Erica, *daboecii*, racemo terminali, foliis alternis ſubtus tomentoſis. Lin. Spec. plant. 509. 38.

Erica Hibernica, foliis myrti piloſis ſubtus incanis. Pet. gaz. 42. tab. 27. fig. 4.

2468 Fragaria, *ſterilis*, caule decumbente repente. Lin. Spec. plant. 709. 3.

Fragaria ſterilis. Bauh. pin. 327.

2469 Gardenia, *Florida*. Lin. Spec. plant. 305. 1.

Jaſminum

Jasminum, foliis lanceolatis oppositis integerrimis, calycibus acutioribus. Miller's Icons, tab. 180.

2470 Genista, *Anglica*, spinis simplicibus, ramis floriferis inermibus, foliis lanceolatis. Lin. Spec. plant. 999. 11.

Genista minor aspaltoides. Bauh. pin. 395.

2471 Heuchera, *Americana*. Lin. Spec. plant. 328. 1.

Sanicula, s. Cortusa Americana spicata, floribus squalide purpureis. Pluk. alm. tab. 58. fig. 3.

2472 Hyssopus, *officinalis*, spicis foecundis. Lin. Spec. plant. 796. 1. α.

Hyssopus officinarum cærulea, s. spicata. Bauh. pin. 217.

2473 Jasminum, *officinale*, foliis oppositis pinnatis. Lin. Spec. plant. 9. 1.

Jasminum vulgatius flore albo. Bauh. pin. 397.

2474 Knautia, *orientalis*. Lin. Spec. plant. 146.

Scabiosa orientalis, caryophylli flore. Vaill. act. 1772. p. 241.

2475 Ligusticum, *Levisticum*, foliis multiplicibus, foliolis superne incisis. Lin. Spec. plant. 359. 1.

Ligusticum vulgare. Bauh. pin. 157.

2476 Medicago, *lupulina*, spicis ovalibus, leguminibus reniformibus monospermis, caule procumbente. Lin. Spec. plant. 1097. 7.

Trifolium pratense luteum capitulo breviore. Bauh. pin. 228.

2477 Mercurialis, *annua*, caule brachiato, foliis glabris, floribus ſpicatis. Lin. Spec. plant. 1465. 3.

Mercurialis teſticulata, ſ. mas. & Bauh. pin.
Mercurialis ſpicata, ſ. fœmina. 121.

2478 Mercurialis, *perennis*, caule ſimpliciſſimo foliis ſcabris. Lin. Spec. plant. 1465. 1.

Mercurialis montana teſticulata, & Bauh. pin.
Mercurialis montana ſpicata. 122.

2479 Petiveria, *alliacea*, floribus hexandris. Lin. Spec. plant. 486. 1.

Verbenæ aut Scorodoniæ affinis anomala, flore albido, calyce aſpero, allii odore. Sloan. Hiſt. I. 172.

2480 Pæonia, *fœmina*, foliolis oblongis. Lin. Sp. plant. 747. 1. α.

Pæonia communis, ſ. fœmina. Bauh. pin. 323.

3481 Pteris, *aquilina*, frondibus ſupradecompoſitis, foliolis pinnatis: pinnis lanceolatis: infimis pinnatifidis, ſuperioribus minoribus. Lin. Spec. plant. 1533.

Filix ramoſa major, pinnulis obtuſis non dentata. Bauh. pin. 357.

2482 Quercus, *Ilex*, foliis ovato-oblongis indiviſis ſerratiſque, petiolatis, ſubtus incanis, cortice integro. Lin. Spec. plant. 1412. 3. β.

Ilex folio anguſto non ſerrato. Bauh. pin. 424.

2483 Quercus, *Suber*, foliis ovato-oblongis indiviſis ſerratis ſubtus tomentoſis, cortice ramoſo fungoſo. Lin. Spec. plant. 1413. 5.

Suber latifolium ſempervirens. Bauh. pin. 424.

2484 Ra-

2484 Ranunculus, *Auricomus*, foliis radicalibus reniformibus crenatis inciſis, caulinis digitatis linearibus, caule multifloro. Lin. Spec. plant. 775. 13.

Ranunculus nemoroſus, ſ. ſylvaticus, folio ſubrotundo. Bauh. pin. 178.

2485 Ranunculus, *Flammula*, foliis ovato-lanceolatis petiolatis, caule declinato. Lin. Spec. plant. 773. 1. α.

Ranunculus longifolius paluſtris minor. Bauh. pin. 180.

2486 Rheum, *palmatum*, foliis palmatis acuminatis. Lin. Spec. plant. 531. 3.

Rhabarbar. Brun. orient. 192. tab. 73.

2487 Rubus, *arcticus*, foliis ternatis, cane inermi unifloro. Lin. Spec. plant. 708. 11.

Rubus caule unifloro, foliis ternatis. Lin. Fl. Lap. 207. tab. 5. fig. 2.

2488 Ruſcus, *Androgynus*, foliis margine floriferis. Lin. Spec. plant. 1474.

Ruſcus latifolius è foliorum ſinu florifer & baccifer. Dil. Hort. Elth. 330. tab. 250. fig. 332.

2489 Salvia, *nemoroſa*, foliis cordato-lanceolatis ſerratis planis, bracteis coloratis, corollæ labio infimo reflexo. Lin. Spec. plant. 35. 10.

Horminum ſylveſtre, ſalvifolium minus. Bauh. pin. 239.

2490 Scutellaria, *albida*, foliis ſubcordatis ſerratis rugoſis opacis, ſpicis ſecundis bracteis ovatis. Lin. Mantiſſa. 248.

Scutellaria Teucrii facie flore albo. Bauh. hiſt. III. 291.

2491 Serratula, *ſpicata*, foliis linearibus baſi ciliatis, floribus ſpicatis ſeſſilibus lateralibus, caule ſimplici. Lin. Spec. plant. 1147. 11.
Jacca anguſtifolia, tuberoſa radice Virginiana. Pluk. alm. tab. 424. fig. 6.

2492 Sibthorpia, *peregrina*, foliis reniformibus crenatis, pedunculis geminis. Lin. Spec. plant. 880. 3.
Planta anonyma. Pluk phyt. tab. 257. fig. 5.

2493 Staphylea, *pinnata*, foliis pinnatis. Lin. Spec. plant. 386. 1.
Piſtacia ſylveſtris. Bauh. pin. 401.

2494 Taxus, *baccata*, foliis approximatis. Lin. Spec. plant. 1472. 1.
Taxus. Bauh. pin. 505.

2495 Teucrium, *hircanicum*, foliis cordato-oblongis obtuſis, caule brachiato dichotomo, ſpicis longiſſimis terminalibus ſeſſilibus ſpiralibus. Lin. Spec. plant. 789. 16.
Teucrium foliis cordatis crenatis petiolatis, ſpicis oblongis denſiſſimis. Arduin. Spec. 13. tab. 4.

2496 Teucrium, *Scordium*, foliis oblongis ſeſſilibus dentato-ſerratis, floribus geminis lateralibus pedunculatis, caule diffuſo. Lin. Spec. plant. 790. 20.
Scordium. Bauh. pin. 247.

2497 Thymus, *Acinos*, verticillis ſexfloris, caulibus erectis ſubramoſis, foliis acutis ſerratis. Lin. Spec. plant. 826. 4.
Clinopodium arvenſe, ocymi facie. Bauh. pin. 225.

2498 Thy-

2498 Thymus, *Serpyllum*, floribus capitatis, caulibus decumbentibus, foliis planis obtusis, basi ciliatis. Lin. Spec. plant. 825. 1. ε.

Serpyllum foliis citri odore. Bauh. pin. 220.

2499 Thymus, *vulgaris*, erectus, foliis revolutis ovatis, floribus verticillato-spicatis. Lin. Spec. plant. 825. 2. α.

Thymus vulgaris folio tenuiore. Bauh. pin. 219.

2500 Viola, *odorata*, acaulis, foliis cordatis, stolonibus reptantibus. Lin. Spec. pl. 1324. 8.

Viola martia purpurea, flore simplici odore. Bauh. pin. 199.

VI. *Extract of a Letter from Mr.* Ebenezer Kinnersley *to* Benjamin Franklin, *LL. D. F. R. S. on some electrical Experiments made with Charcoal.*

Received August 16, 1772.

Philadelphia, October 13, 1770.

Read Dec. 10, 1772.

THE conducting quality of some sorts of charcoal is indeed very remarkable. I have found oak, beech, and maple, to conduct very well; but tried several pieces of pine coal, without finding one that would conduct at all; perhaps they were made in a fire not hot enough, or not continued in it long enough. A strong line drawn on paper with a black lead pencil, will conduct an electrical shock pretty readily; but this, perhaps, may not be new to you.

On the 12th of last July, three houses in this city, and a sloop at one of the wharfs, were, in less than an hour's time, all struck with lightning. The sloop, with two of the houses, were considerably damaged; the other was the dwelling-house of Mr. Joseph Moulde, in Lombard-street, which was provided with

with a round iron conductor, half an inch thick, its feveral lengths fcrewed together, fo as to make very good joints, and the lower end five or fix feet under ground; the lightning, leaving every thing elfe, purfued its way through that, melted off fix inches and a half of the flendereft part of a brafs wire fixed on the top, and did no further damage within doors, or without. Captain Falconer, who brings you this, was in the houfe at the time of the ftroke, and fays it was an aftonifhing loud one.

VII. *Account of an Experiment made with a Thermometer, whose Bulb was painted black, and exposed to the direct Rays of the Sun: In a Letter from* Richard Watson, *D. D. Regius Professor of Divinity at* Cambridge, *and F. R. S. to* Mathew Maty, *M. D. Sec. R. S.*

Heversham, September 18, 1772.

DEAR SIR,

Read Dec. 1, 1772.

DURING the hot weather, which we had in the latter end of June and the beginning of July laſt, I made an experiment at Cambridge, which I then thought no more of, but which an accident hath brought to my mind again; and I now venture to ſend you an account of it, in hopes that ſome of your philoſophical friends will take the trouble of proſecuting it. I expoſed the bulb of an excellent thermometer to the direct rays of the Sun, when the ſky was perfectly free from clouds: the mercury roſe to 108° of Fahrenheit's ſcale, and continued ſtationary. A fancy ſtruck me, to give the bulb a black covering; this was eaſily effected by a camel's hair pencil and Indian ink; the mercury

mercury ſunk a few degrees during the application of the coating, and the evaporation of the water; but presently after roſe to 118°, or 10° in conſequence of the black coat with which I had covered that part of the bulb which was expoſed to the Sun. If the bulbs of ſeveral correſponding thermometers were painted of different colours, and expoſed at the ſame time to the Sun, for a given period, ſome conjectures, reſpecting the diſpoſition of the ſeveral primary colours for receiving and retaining heat, might be formed, which could not fail of being, in ſome degree, intereſting.

I am,

DEAR SIR,

Your moſt obedient ſervant,

Richard Watſon.

VIII. *A Report of the Committee appointed by the Royal Society, to consider of a Method for securing the Powder Magazines at* Purfleet.

To the PRESIDENT and MEMBERS of the ROYAL SOCIETY.

GENTLEMEN,

THE Society being consulted by the Board of Ordnance, on the propriety of fixing conductors for securing the powder magazines at Purfleet from lightning, and having thereupon done us the honour of appointing us a committee, to consider the same, and report our opinion; we have accordingly visited those buildings, and examined, with care and attention, their situation, construction, and circumstances, which we find as follows.

They are five in number, each about 160 feet long, and about 52 feet wide, built of brick, arched under the roof, which in one of them is slated, with a coping of lead 22 inches wide on the ridge from end to end; and the others, as we were informed, are

are ſoon to be covered in the ſame manner. They ſtand parallel to each other at about 57 feet diſtance, and are founded on a chalk rock, about 100 feet from the river, which riſes in high tides within a few inches of the level of the ground, its brackiſh water alſo ſoaking through to the wells that are dug near to the buildings.

The barrels of powder, when the magazines are full, lie piled on each other up to the ſpring of the arches; and there are four copper hoops on each barrel, which, with a number of perpendicular iron bars, (that came down through the arches, to ſupport a long grooved piece of timber, wherein the crane was uſually moved and guided to any part where it was wanted) formed broken conductors within the building, the more dangerous from their being incompleat, as the exploſion from hoop to hoop, in the paſſage of lightning drawn down through the bars among the barrels, might eaſily happen to fire the powder contained in them. But the workmen were removing all thoſe iron bars (by the advice of ſome members of this Society, who had been previouſly conſulted); a meaſure we very much approve of.

On an elevated ground, nearly equal in height with the tops of the magazines, and 150 yards from them, is the houſe where the Board uſually meet. It is a lofty building, with a pointed hip-roof, the copings of lead down to the gutters, from which leaden pipes deſcend at each end of the building into the water of wells of 40 feet deep, for the purpoſe of conveying water forced up by engines to a ciſtern

in the roof. There is alſo a proof-houſe, adjoining to the end of one of the magazines, and a clock-houſe, at the diſtance of feet from them, which has a weathercock on an iron ſpindle, and, probably, ſome incompleat conductors within, ſuch as the wire uſually extending up from a clock to its hammer, the clock, pendulum, rod, &c.

The blowing up of a magazine of gun-powder by lightning, within a few years paſt, at Breſcia in Italy, which demoliſhed a conſiderable part of the town, with the loſs of many lives, does, in our opinion, ſtrongly urge the propriety of guarding ſuch magazines from that kind of danger; and ſince it is now well known, from many obſervations, that metals have the property of conducting lightning; and a method has been diſcovered of uſing that property for the ſecurity of buildings, by ſo diſpoſing and fixing iron rods, as to receive, and convey away, ſuch lightning as might otherwiſe have damaged them; which method has been practiſed near twenty years in many places, and attended with ſucceſs, in all the inſtances that have come to our knowledge, we cannot, therefore, but think it adviſeable to provide conductors of that kind for the magazines in queſtion.

In common caſes, it has been judged ſufficient, if the lower part of the conductor were ſunk three or four feet into the ground, till it came to moiſt earth; but this being a caſe of the greateſt importance, we are of opinion that greater precaution ſhould be taken. Therefore, we would adviſe, that, at each end of each magazine, a well ſhould be dug

in,

in or through the chalk, ſo deep as to have in it at leaſt four feet of ſtanding water. From the bottom of this water ſhould ariſe a piece of leaden pipe, to or near the ſurface of the ground, where it ſhould be ſtrongly joined to the end of an upright iron bar, an inch and half diameter, faſtened to the wall by leaden ſtraps, and extending ten feet above the ridge of the building, tapering from the ridge upwards to a ſharp point, the upper 12 inches of copper, the iron to be painted.

We mention lead for the under-ground part of the conductor, as leſs liable to ruſt in water and moiſt places; in the form of a pipe, as giving greater ſtiffneſs for the ſubſtance; and iron for the part above-ground, as ſtronger, and leſs likely to be cut away. The pieces, of which the bar may be compoſed, ſhould be ſcrewed ſtrongly into each other, by a cloſe joint, with a thin plate of lead between the ſhoulders, to make the joining or continuation of the metal more perfect. Each rod, in paſſing above the ridge, ſhould be ſtrongly and cloſely connected by iron or lead, or both, with the leaden coping of the roof, whereby a communication of metal will be made between the two bars of each building, for a more free and eaſy conducting of the lightning into the earth.

We alſo adviſe, in conſideration of the great length of the buildings, that two wells, of the ſame depth with the others, ſhould be dug within twelve feet of the doors of the two outſide magazines; that is to ſay, one of them on the north ſide of the north building, the other on the ſouth ſide of the ſouth build-

building; from the bottom of which wells, ſimilar conductors ſhould be carried up to the eaves, there joining well with a plate of lead, extending on the roof up to the leaden coping of the ridge, the ſaid plate of lead being of equal ſubſtance with that of the coping.

We are further of opinion, that it will be right to form a communication of lead from the top of the chimney of the proof-houſe to the lead on its ridge, and thence to the lead on the ridge of the corridor, and thence to the iron conductor of the adjacent end of the magazine; and alſo to fix a conductor from the bottom of the weather-cock ſpindle of the clock-houſe, down on the outſide of that building, into the moiſt earth.

As to the board-houſe, we think it already well furniſhed with conductors, by the ſeveral leaden communications above-mentioned, from the point of the roof down into the water, and that, by its height and proximity, it may be ſome ſecurity to the building below it; we therefore propoſe no other conductor for that building, and only adviſe erecting a pointed iron rod on the ſummit, ſimilar to thoſe before deſcribed, and communicating with thoſe conductors.

To theſe directions we would add a caution, that, in all future alterations or repairs of the buildings, ſpecial care be taken that the metalline communications be not cut off or removed.

It remains that we expreſs our acknowledgements to Sir Charles Frederick, Surveyor-general of the Ordnance, for the obliging attention with which he entertained

entertained and accommodated us on the day of our enquiry.

With very great reſpect, we are,

GENTLEMEN,

Your moſt obedient,

humble ſervants,

Auguſt 21, 1772.

H. Cavendiſh,
William Watſon,
B. Franklin,
J. Robertſon.

Mr. Wilson's *Dissent to Part of the preceding Report.*

I Dissent from the Report above, in that part only which recommends that each conductor should terminate in a *point*.

My reason for dissenting is, that such conductors are, in my opinion, less safe than those which are not *pointed*.

Every *point*, as such, I consider as *solliciting* the lightning, and, by that means, not only contributing to *increase* the quantity of every actual discharge, but also frequently occasioning a discharge where it might not otherwise have happened.

If, therefore, we invite the lightning, while we are ignorant what the quantity, or the effects of it, may be, we may be *promoting* the very mischief we mean to prevent.

Whereas if, instead of pointed, we make use of blunted conductors, those will as effectually answer the purpose of conveying away the lightning *safely*, without that tendency to *increase* or *invite* it.

My further reasons for disapproving of *points*, in all cases, where conductors are judged necessary, are contained in a letter addressed to the Marquis of Rockingham, and published in the Philosophical Transactions, Vol. LIV. p. 247.

There are other reasons also, which I have to offer, for rejecting points on this *particular occasion*; and which, *were mentioned at the committee*. Those I shall lay before the Royal Society at another opportunity, for the benefit of the publick.

Royal Society House,
August 21, 1772.

Benj. Wilson.

IX. *Obser-*

IX. *Obſervations upon Lightning, and the Method of ſecuring Buildings from its Effects: In a Letter to Sir* Charles Frederick, *Surveyor-General of His Majeſty's Ordnance, and F. R. S. By* Benjamin Wilſon, *F. R. S. & Ac. R.* Upſ. *Soc.*

S I R,

YOUR ſtation, as Surveyor-General of His Majeſty's Ordnance, being ſuch, as makes the ſubject of this paper particulary intereſting to you, I preſume an apology for this addreſs will be wholly unneceſſary.

Upon an application of the Board of Ordnance to the Royal Society, in July laſt, a committee was appointed, to conſider of the propereſt method for ſecuring the *Magazine* at *Purfleet* from miſchief by lightning: which committee reported to the council of that learned body, what they thought neceſſary to be done upon that occaſion. The council, afterwards, tranſmitted to the board, a copy of that report, together with another paper written by myſelf, in conſequence thereof.

 For,

For, during the consideration of that business, some doubts having arisen in my mind, with regard to the propriety of *points*, which were proposed to terminate the top of each conductor; and those doubts being founded upon experiments and observations, I could not consistently subscribe to that report, nor suppress my opinion, on a subject of such importance.

Whatever may be the sentiments of others respecting those doubts, yet, they being the result of my mature consideration, I thought it my duty to propose them to the committee; and further to express my dissent, in writing, to that particular part of their report: giving, at the same time, some of the principal reasons for such dissent; and referring them, for further satisfaction on this subject, to a letter which is already published in the Transactions of the Royal Society.

As that dissent is the origin of this paper, a copy of it is here inserted. See page 48 of this Volume.

AGREEABLE to the declaration at the end of the above dissent, I shall now proceed to offer my further reasons for objecting to pointed conductors.

Experience, which is our best guide in all physical enquiries, but particularly in electrical ones, every day convinces me, that we know but little of that subtile fluid, which operates so *secretly*, and at the same time so powerfully, upon the earth, and its atmosphere I confess that I am even now less acquainted with the principle of its action, than I thought I was twenty years ago: the *smallest differences*

ferences in the circumstances of our experiments, frequently causing very material differences in their results. And perhaps no one, who has not applied his mind closely to enquiries of this kind, could conceive how the *pointing* a piece of metal, or *not*, should make any material difference in the experiment.

The electrician has it always in his power to convince any one of the fact, who, through inexperience, may be inclined to entertain the least scruple about it: for *even from those experiments* to which it was thought proper to appeal at the committee, it appeared, that the difference in the effects upon this fluid, between *pointed* and *blunted* metal, is as 12 to 1.

A thunder-cloud therefore, according to that reasoning, (the circumstances of it being supposed to be nearly similar with what is called the prime-conductor in those experiments), if it acted at 1200 yards distance upon a point, would require a blunted end to be brought within the distance of 100 yards; and beyond those limits, would pass over it, without affecting it at all. On this occasion permit me to observe, that the *longer* the conductors are above any building, the *more danger* is to be apprehended from them; as they will in that case approximate nearer in their effects to those that are pointed. And that is one reason why I was not for advising the proposed conductors at *Purfleet*, to be so high as *ten feet* above the magazines, and more particularly upon that building called the *Board-house*, which stands considerably higher than the magazines themselves.

But, before we advance farther into this subject, it may be proper to shew the reasons for introducing a

pointed apparatus, when the experiment upon lightning was firft propofed: what good confequences were derived from that experiment: and why, upon further experiments and obfervations, fuch points ought now to be laid afide, when our intention is *not to make electrical experiments*, but by the means of conductors, *to preferve buildings from the dangerous effects of lightning*.

Dr. Franklin, in his conjectures, that lightning and electricity were one and the fame fluid, confidered how he fhould *invite*, or *bring down and collect the lightning*, fo as to make experiments upon it.

And he concluded, from obfervation, that the likelieft method would be, to make ufe of fuch an apparatus for the purpofe, as was moft fufceptible of electric effects; or, in other words, fuch an apparatus as would receive the electric fluid with the greateft eafe.

Repeated experiments taught him, that *metals* had the property of receiving that fluid, with more eafe than other fubftances.

He alfo learnt, from the like experience, that metals *by being pointed*, were rendered ftill more fufceptible of receiving it.

And therefore, he propofed an experiment to be tried, "Whether it was not in our power to invite, "or bring down the lightning, by an apparatus, "confifting of an *electric ftand*, and an iron rod, "twenty or thirty feet in length, rifing upright "from the middle of the ftand, and at the top, "terminating in a very *fharp point*." This apparatus was recommended to be put upon fome high building,

building, with the expectation, that if a thunder-cloud should happen to pass near this apparatus, some quantity of the lightning deposited therein would probably be collected in the rod, by means of the very sharp point, and the electrical stand at the foot of the rod

That this contrivance answered the end he first proposed, we have had sufficient evidence.

And it is no wonder if, after this great discovery, we find him, and other electricians, pursuing new experiments of this kind, and raising those points higher into the air, to collect still greater quantities of that fluid which occasions lightning. Nor need we be surprized, after knowing that lightning could be brought down from the heavens by so simple an apparatus, and after experiencing its subtile effects to be similar with the electric fluid, that the Americans, and others, upon Dr. Franklin's recommendation, adopted the principle of securing their buildings from its dangerous effects, by raising above their houses rods of iron, very *sharply pointed*, and applying *wires* from the ends of those rods, down the outside of their houses, to the ground.

But though there appeared many arguments at that time in favour of such conductors, yet experiments and observations, at last, induced Dr. Franklin to *alter his opinion* in respect to those *wires*, and to substitute in their place *rods of iron*: still retaining the principle of having the rods at the top *sharply pointed*; and many of the Americans, as well as Europeans, approved of the alteration, as appeared afterwards, from constructing their conductors accordingly.

About

About that time great attention was given, and many new experiments were made, in consequence of the frequent dangerous effects, which lightning was observed to produce in some valuable buildings, by rending and [illegible]ing to pieces very large ſtones and timbe[illegible]were connected together by cramps and bars of iron: and at other times breaking and melting part of thoſe rods, and ſometimes *exploding wires*, even of a conſiderable thickneſs, like ſo much gun-powder.

From careful obſervations of theſe extraordinary appearances produced by violent ſhocks of lightning; and upon making other experiments relating to a certain reſiſting power *in*, or *upon*, all bodies, which appears to act againſt the attacks of lightning, as well as againſt the electric fluid, philoſophers were enabled to aſſign the reaſon, and, it is apprehended, upon a ſolid foundation, why Conductors ſhould be made of *metal*, in preference to all other materials; as the power of reſiſting ſuch attacks is leſs in metals than in wood, ſtone, or marble.

And that this reſiſtance might be the more ſimple and uniform, it appeared the moſt eligible to have the conductors made of *one continued piece of metal only*, and of *an equal diameter throughout*. But what that diameter ought to be, depended upon other circumſtances, ſome of which are taken notice of in a former paper, referred to above, which I laid before the Royal Society.

By this hiſtorical ſketch, we ſee the propriety of Dr. Franklin's introducing points, and the advantage philoſophy has derived from them: by aſcertaining

that lightning and electricity are one and the fame fluid: which appears to be diffused every where, at leaft upon *this earth* and in *the atmofphere*.

But when *curiofity*, which I apprehend was one of the firft motives for introducing points to invite the lightning, was *fatisfied*; and *experience* had taught us, that we had it in our *power* to *collect* that fluid which occafions it: and when the *principle* of its action was from experiments thus inveftigated and *afcertained*, this manner of *invitation*, viz. by ufing points, ought, in my opinion, to have *ceafed* *; becaufe a greater quantity of lightning, than we have yet experienced, may chance to attack us.

For we are fo far from knowing how great the magazine of lightning may be in the heavens, or in the earth, when it is ready to difcharge itfelf, either by one or more explofions, that we are ignorant, *even of the quantity actually difcharged*, whenever any ftroke from lightning vifits us.

Nor can the ableft philofopher *fix the limits* of the *greateft difcharge* that may poffibly happen.

Seeing then how vain it is to look for any thing like *abfolute fecurity*, in all cafes, it furely behoves us to proceed with *caution*. And it is for that reafon I have always confidered pointed conductors as being *unfafe*, by their great readinefs to *collect the lightning in too powerful a manner*. And left the conductors, without fuch points, fhould be too flender for very

* Unlefs where the electrician, like *Profeffor Richmann* (who was killed by it) at his *own hazard*, chufes to make further obfervations on lightning.

violent

violent attacks, in places of great conſequence, I have always recommended the having them above *four times* larger in diameter, than what are commonly made uſe of, to the end our ſecurity may be the greater, by opening a larger paſſage for any extraordinary diſcharge, and ſo far leſſening the danger to be apprehended from it.

I ought not, in this place, to omit taking notice of a paper, containing ſome further experiments and obſervations, which were produced at the committee, to ſhew, among other things, that pointed metals were more diſpoſed to receive the lightning, by virtue of a *repelling principle* in the lightning, as well as the electric fluid, which acted upon the *nâtural quantity* of the fluid contained within the metal, at a conſiderable diſtance from the *point*, cauſing, if I may be allowed the expreſſion, a kind of *vacuum* therein; but I ſuppoſe the author means to a certain diſtance only.

So far from diſputing this philoſophy, I readily admit the fact.

But, I am afraid, *every attempt* to prove that pointed conductors may be ſo diſpoſed to receive this fluid more readily, will not mend the argument in the leaſt; becauſe, the more we leſſen the power of reſiſting, even ſuppoſing the whole Conductor to be in that ſtate, the more we increaſe the power of invitation.

In regard to other experiments, with *locks of cotton* *, which are acted upon in a particular manner

* Dr. Franklin's Experiments.

by the appoſition of points, and the concluſions drawn from thence, in favour of pointed conductors, as cauſing ſimilar effects upon the *fragments or ſmall clouds*, which, hanging below the thunder-clouds, have been ſuppoſed a kind of *ſtepping-ſtones*, for the lightning to paſs upon, towards the earth: ſuch pointed conductors being ſuppoſed to occaſion thoſe *fragments* to *retire up into the cloud* from whence they were ſuſpended; and on that account, to prevent a ſtroke from lightning, which might otherwiſe have happened, I ſhall, for the preſent, wave entering into this philoſophy, as I could wiſh the conjecture to be reconſidered; becauſe I apprehend it is liable to many objections, which to enumerate would carry me beyond the proper bounds of ſuch a paper as this. However, if the ſame opinion ſhould again be offered, and brought in argument, it may be worth while to enter more *deeply* into the enquiry.

If thoſe gentlemen, who argued at the committee for the *neceſſity of points*, could have made it appear, that ſuch points draw off, and conduct away, the lightning *imperceptibly* and *by degrees*, *without cauſing any exploſion*, during a thunder ſtorm (which ſeems to have been once the opinion of Dr. Franklin) I ſhould readily have ſubſcribed to their Report.

But experience ſhews us, that the fact is otherwiſe: there being many inſtances, where violent exploſions of lightning have happened to *conductors* that were *ſharply pointed*. And *three* in particular, the accounts of which are inſerted in a publication of Dr. Franklin's *, where the *points*

* Dr. Franklin's Experiments, p. 394. 416, 417, &c.

were *diſſipated*, or *deſtroyed*; and a ſmall part of an iron rod melted next the points of one of them; and alſo at the ſeveral crooked ends of the rods below, where they were hooked on to each other, and formed the conductor belonging to Mr. Maine in North America. But as thoſe letters are long, and contain ſeveral other curious facts, I ſhall reſerve them, together with ſome further obſervations upon the *nature* and *power* of that *reſiſting principle*, which is found to act ſo ſenſibly againſt the attacks of the electric fluid, or lightning, to ſome future diſſertation.

There is no building, that I know of, more expoſed to this kind of danger, than the *Eddyſtone Lighthouſe*, as it ſtands upon a rock in the ſea, ſeveral miles from land. The fixing of a conductor to that building, was thought highly proper; and the fixing of a *point* upon it, as highly improper. It was therefore reſolved upon to put up a *conductor without a point*, that no more lightning might be unneceſſarily ſollicited to the building, and that all the lightening, which accidentally fell on it, might be conveyed away without injuring it. This conductor was *fixed* twelve years ago, and the building has ſince received no injury from lightning *.

There is another edifice of great conſequence, I mean St. *Paul's Church*, which ſtands much expoſed, from its heighth, to accidents by lightning. The dean and chapter of that cathedral, thought it an object deſerving the ſerious attention

* N. B. A former building erected for the ſame purpoſe, upon this rock, was ſet on fire by lightning.

of the *Royal Society*. A committee was therefore appointed, in conſequence of their application: and proper *conductors* were put up, in the ſeveral places where they were thought neceſſary, from the top of the *lanthorn* to the *ſewers* underground. And notwithſtanding particular care was taken, to have the additional metal either of a conſiderable diameter, or an equal quantity of it formed into other ſhapes, for the conveniency of the ſeveral places; yet part of thoſe conductors, conſiſting of iron, in the *ſtone gallery*, ſhewed marks of their having been made *conſiderably hot*, if not abſolutely *red*, by a ſtroke of lightning which happened in March laſt (as appears by a letter which I communicated to the Royal Society from one of the vergers of that church, Mr. Richard Gould) who had examined the conductors the morning following, along with Mr. Burton of the ſame cathedral *, and that the appearances were in general,

* Mr. Gould acquaints us in his letter, that he examined the four conductors in the lanthorn and ſtone gallery of St. Paul's Church, the morning after the lightning happened. That no marks whatſoever appeared upon the conductor to the *ſouth*, which was the firſt he attended to. That he examined next the conductor to the *Weſt*, and obſerved a *thick ruſt* lying upon the pavement in the ſtone gallery, as if it had been cleaned off, from the conductor, with a tool: that ſeveral parts of the iron appeared *black*, particularly the ſcrews or nuts: ſomething like the effects left by gun-powder upon iron or ſteel, or a ſmoaky fire.

That the conductor to the *North*, ſhewed no marks, no more than that to the South.

But that upon examining the conductor to the *Eaſt*, he found ſtronger marks *abundantly*, than on the *Weſt* conductor, it

ral, as the verger's letter related them to me. Mr. Delaval and myself attended, about a week afterwards, to observe them, and their particular

situations,

it being *much blacker*; particularly on the nuts and screws: the rust lying in greater quantities on the pavement. *And the extreme part of the conductor that goes into the water trunk*, seemed like a piece of iron *newly taken out of a forge* by a smith, without working it on the anvil.

N. B. Mr. Gould has since added to the account in his letter, some circumstances which I apprehend ought not to be omitted. He says, that where the end of the conductor, on the East side, points towards the water trunk, a stone surrounds part of it, leaving an interval, half an inch wide or more, between them, and about four or five inches long, which is a little more than the breadth of the conductor. That this interval was *filled up with dirt*, and had been so for some time, occasioned by frequent showers of rain washing the pavement in the stone-gallery. That, after the lightning happened, he observed a *hole* was made through the *dirt*, *one quarter of an inch in diameter*, and about *two inches in length*. That the hole was close to the iron; and that, upon stooping down his head, he perceived a very disagreeable *smell of sulphur* from the stone, dirt, and conductor, particularly the last.

Upon hearing this account, Mr. Delaval and myself, a few days ago, went and examined the conductors again; but more carefully than before. For, upon causing the stone to be removed, which covered the top of the water trunk, we had an opportunity of examining near two feet more of the iron which points to the water trunk, than we could perceive before this stone was removed. When we observed, that the conducting iron did not touch the lead. We likewise observed, that there was a *very thick coat of rust* all over that part of the iron; *particularly at the end next the lead*, where the water entered the trunk.

As the necessity of attending to these circumstances will be obvious to any one, who is but in the least degree acquainted with these researches, *the danger of neglecting them will be seen in the strongest light*, *by the gentlemen of the committee* who recommended the conductors for the security of that Cathedral.

ſituations; with the circumſtances attending them, when we were very well ſatisfied with his account, notwithſtanding it had rained in the *interim* for three days together.

It is worthy of note, that thoſe conductors did *not terminate in a point*, nor was any *point put* upon the *croſs at the top*.—And yet Dr. Franklin was of that committee.

If points are ſo eſſential to our ſafety, why was not the reaſon enforced at the committee, for having them on that capital edifice? For my part, I think it was a happy circumſtance, that there was *no point* fixed upon the top of the church, to ſollicit a greater quantity of lightning at that moment, than what fell upon the conductors, circumſtanced as they were: as *that quantity* was great enough to *beat ſo conſiderably* a *bar of iron*, near *four inches broad*, and about *half an inch thick*.

This powerful effect, reminds me of another inſtance ſtill more extraordinary, which happened in Martinico, and is related by *Captain Dibden*, where a bar of iron, *one inch in diameter*, was by a violent ſhock of lightning *reduced in one part of it, to the thickneſs of a ſlender wire only*. See Ph. Tr. Vol. LIV. p. 251.

Since then we are at all times ignorant of the quantity of *lightning in the* earth and its atmoſphere; and the difference in the effects, between blunted and pointed, ends, in cauſing a diſcharge in our electrical experiments, appears to be as *one to twelve*; it is eaſy to comprehend the very great danger this noble fabrick has probably eſcaped, by having no pointed apparatus upon it.

From

From the above obſervations, I am naturally led to conſider a part of the proceedings of the committee, reſpecting the *magazines* at *Purfleet*; when a certain number of conductors, with tapering points at the top, were reſolved upon, as neceſſary, to protect the ſeveral buildings where the powder is depoſited. For it was *agreed* upon at the ſame meeting, that the *Board-houſe*, which is a large building for the uſe of the board-officers, and which ſtands conſiderably higher than the magazines, as was obſerved above, *did not require any point at the top*: becauſe it was apprehended to be *perfectly ſecure*, by reaſon of the copeings on the roof, the gutters and pipes to carry off the water, being all of lead: and further, becauſe thoſe pipes communicated with two wells, which always contained water.

I was not a little ſurprized at this laſt reſolution, which appeared to be ſo inconſiſtent with the former. Becauſe, if points were neceſſary in one place, they ought to be ſo in another. And on the other hand, if the Board-houſe is ſecure by the leaden accidental conductors, which have no points, why ought not the magazines to be equally ſecure, when put into the ſame circumſtances?

I therefore enforced the *inconſiſtency* of ſuch a reſolution in the ſtrongeſt terms. Notwithſtanding which, the gentlemen, at that time, thought proper to confirm their reſolution. However, at the next meeting of the committee, I obſerved that they had been pleaſed, in the mean time, to make an amendment in favour of points for the board-

houſe:

houſe; which amendment was no ſooner propoſed, than approved of.

Why my obſervation was rejected at the preceding meeting, I muſt leave to the judgment of others. But it certainly carries an appearance, as if *manifeſt contradiction*, upon further reflection, muſt have been the *cauſe* of that alteration.

And I am inclined to believe, from ſome gentlemen of the committee expreſſing their opinion, "*of its being a matter of mere indifference whether* "*blunted or pointed conductors were made uſe of,*" that they have not conſidered this ſubject, with all the due attention which ſo important an object deſerves.

For if our experiments ſhew, that *points*, from the *nature* of their *ſhape*, and *other* circumſtances attending them, reſiſt the attacks of this fluid leſs than blunted ones; and that blunted conductors, of proper dimenſions, are ſufficient to convey away the lightning *ſafely*, whenever it attacks them; why ſhould we have recourſe to a method, which is at beſt uncertain; and which ſome time or other may be productive of the moſt fatal effects?

But perhaps no argument can be brought with more force againſt the principle of points, than Dr. Franklin's own words, which are publiſhed in his experiments, p. 481, where he declares *poſitively*. "Buildings, that have their roofs covered with "lead, or other metal, and ſpouts of metal conti-"nued from the roof into the ground to carry off "the water, *are never hurt by lightning*; as when-"ever it falls on ſuch a building, it paſſes in the "metals, and not in the walls."

This

This is the case with the *British Museum*, a building also of considerable consequence, where there are no other *conductors*, than what are formed by the *copeings, gutters and pipes*, which are all of lead, and communicate with the ground. Now it is from the great quantity of metal contained in the several pipes, together with the other circumstances attending them, that I considered that building (in a former paper laid before the royal society) as being sufficiently secured, from those dangerous accidents.

But if any gentleman should be disposed to entertain a doubt about it, or indeed of any other part of my reasoning on this subject, a *declaration* of those doubts may be attended with good consequences, as they will necessarily open the door to a more minute investigation.

I have now, Sir, gone through the reasons which I proposed to lay before the royal society for the rejecting of points. And I am very sorry, in the course of this letter, to have been under the necessity of mentioning any differences in opinion, which passed between the members of the committee, to whom this important matter was referred. I think, however, I shall stand excused to the society, and the public, when it appears, as I hope it now sufficiently does, what my motive has been; namely, to state clearly, and impartially, the objections which I conceived to lye against pointed conductors: and to disclose *without any reserve*, the principles on which such objections are grounded.

I am, SIR, with the greatest respect,

Your most obedient, and most humble servant,

8th December, 1772,
Great Russel-street,
Bloomsbury.

Benjamin Wilson.

P. S.

P. S. Mr. Delaval, who was one of the committee, has given me leave to insert his opinion upon this subject; which is this. That he concurs with me in thinking that such conductors as are elevated higher than the buildings to which they are applied, or are pointed at the top, are *improper* and *dangerous*.

He was desirous of delivering his opinion at the committee: but, as the meetings of it were held in the summer only, his absence from London prevented his attendance.

X. *A Letter to Sir* John Pringle, *Bart. Pr. R. S. on pointed Conductors.*

S I R,

Read Dec. 17, 1772.

HAVING heard and confidered the objections to our Report, concerning the fixing pointed Conductors to the Magazines at Purfleet, contained in a letter from Mr. Wilfon to Sir Charles Frederick, and read to the Royal Society, we do hereby acquaint you, that we find no reafon to change our opinion, or vary from that Report.

We have the honour to be,

S I R,

Your moft obedient,

humble fervants,

December 17, 1772.

H. Cavendifh,
W. Watfon,
B. Franklin,
J. Robertfon.

XI. *Aftro-*

XI. *Astronomical Observations made at* Chislehurst *in* Kent, *by the Reverend* Francis Wollaston, *F. R. S.*

Received November 12, 1772.

Read Dec. 17, 1772.

I the last year did myself the honour to deliver to this Society, an account of the going of an astronomical clock with a wooden perdulum, for the year preceding; together with such observations as I had made in this place; the latitude of which is 51° 24′ 33″ North, and its longitude 4′ 39″ = 18″,6 in time, East of the Royal Observatory at Greenwich. As that account seemed not wholly unacceptable, I have now continued it down to the present time: omitting, however the series of transits; which I gave in the former paper, merely to shew the exactness with which they might be taken with a very small instrument, beyond what I had expected to find.

The rate of the clock deduced from the observations of this last year, will not be found so uniform as the foregoing. To what cause to ascribe it, I am not certain. I think not to heat: perhaps to the great drought of the summer. However, its acceleration or retardation was not desultory, but suf-

 ficient

ficient to be depended upon for any intermediate time. The clock was cleaned in November; and when ſet up again, loſt, between the 18th and 28th, at the rate of 7″,8 per day. The regulator was then altered, and clock ſet, and from that time never meddled with.

My obſervatory (if it deſerves that name) is a room up two pair of ſtairs, in a ſquare compact brick houſe, pretty much expoſed indeed to wind, from our high ſituation. The face of the clock turns due Eaſt. I have ſet down the *throwing-out* of the pendulum, as judged by the eye, on a ſcale behind it divided only to every 10 minutes; and therefore ſome of the variations may only be errors in judgment. How far a clock ſo ſituated may be affected by any motion of the houſe in windy weather, deſerves conſideration. I do not however perceive that it *is*, either in the *rate* or *throwing-out*.

1771.	Clock + too fast — too slow for mean sol. time.	Gain + or Loss —	Number of days.	Rate per day.	Throwing out on the South side.	Throwing out on the North side.
	′ ″	″		″	° ′	° ′
Nov. 29	Clock — 0,6	—17,3	17	—1,02	1 42	1 45
Dec. 16	— 17,9	—12,1	14	—0,86	1 43	1 46
30	— 30,0	— 8,0	14	—0,57	1 43	1 45
1772.						
Jan. 13	— 38,0	— 6,9	22	—0,31	1 43	1 46
Feb. 4	— 44,9	— 9,5	11	—0,86	1 44	1 46
15	— 54,0	— 8,9	17	—0,52	1 42	1 44
Mar. 3	— 1 2,9	— 2,8	32	—0,09	1 42	1 44
April 4	— 1 5,7	+ 2,0	9	+0,22	1 42	1 44
13	— 1 3,7	+48,3	19	+2,54	1 38	1 40
May 2	— 15,4	+42,8	11	+3,89	1 43	1 45
13	+ 27,4	+43,8	10	+4,38	1 42	1 44
23	+ 1 11,2	+41,8	9	+4,66	1 42	1 44
June 1	+ 1 53,0	+61,1	14	+5,08	1 43	1 45
15	+ 2 54,1	+54,8	18	+6,38	1 43	1 45
July 3	+ 4 48,9	+50,5	7	+7,21	1 43	1 45
10	+ 5 39,4	+81,1	11	+7,37	1 43	1 45
21	+ 7 0,5	+78,3	10	+7,83	1 43	1 45
31	+ 8 18,8	+58,1	8	+7,26	1 48	1 50
Aug. 8	+ 9 16,9	+60,0	9	+6,44	1 47	1 49
17	+ 10 16,9	+72,9	11	+6,63	1 47	1 49
28	+ 11 29,8	+52,8	10	+5,28	1 44	1 47
Sept. 7	+ 12 22,6	+31,3	8	+3,91	1 46	1 48
15	+ 12 53,9	+22,0	6	+3,33		
21	+ 13 15,9	+37,1	10	+3,71	1 48	1 50
Oct. 1	+ 13 53,0	+27,6	9	+3,06		
10	+ 14 20,6	+20,0	9	+2,22		
19	+ 14 40,6	+32,4	14	+2,31	1 44	1 48
Nov. 2	+ 15 13,0					

I have

I have this year kept a register of the Thermometer and Barometer; the higheſt and loweſt ſtate of which, as I obſerved them in each month, I will here ſubjoin. I do not find that they have any reference to the rate of the clock; but they may ſerve for compariſon with other places. The Thermometer *A* was made by Mr. Nairne; and hangs without doors, near a window, up one pair of ſtairs, on the North ſide of my houſe; with a ſkreen to keep off the morning ſun in ſummer, but no building near it. The three firſt columns ſhew its ſtate at eight o'clock in the morning, two in the afternoon, and eleven at night. The fourth column gives the Thermometer *B*, in the obſervatory, near the face of the clock, at nine in the morning. The fifth ſhews the Barometer; which being portable, I cannot be certain that its ſcale is exactly placed; though I believe it is right. It is kept in a room on the ground floor.

1771.		Thermometer A. H. 8. Mat.	H. 2. P. M.	H. 11. P. M.	B. H. 9. Mat.	Barometer.
Dec.	Higheſt	52	50	53	52	30,05
	Loweſt	34	35	33	36	28,58
1772.						
Jan.	Higheſt	49	52	49	50	30 09
	Loweſt	22	30	21	29	28,63
Feb.	Higheſt	50	47	48	50	29 92
	Loweſt	25	32	24	32	28,79
Mar.	Higheſt	51	56	46	50	29,78
	Loweſt	27,5	31	27	30	28,79
April	Higheſt	51	59	50	52	30,04
	Loweſt	31	38	31	36	29,19
May	Higheſt	63	70	53	58	30,20
	Loweſt	42	47	33	44	29,35
June	Higheſt	73	82	68	73	30,12
	Loweſt	53	57	49	56	29 50
July	Higheſt	70	76,5	63	68	30,10
	Loweſt	55	61	49,5	58	29,14
Aug.	Higheſt	73	75	67	70	30,08
	Loweſt	52	62	50	60	29,23
Sept.	Higheſt	66	74	66,5	70	29,96
	Loweſt	48	58	46	45	28,82
Oct.	Higheſt	60	66	59	62	30,12
	Loweſt	44	52	43	51	28,88

Eclipſe of the Sun, ⊙ October 25, 1772.

App. time.			
h	′	″	
20	31	12	The firſt impreſſion that I perceived. But I had looked for it on the Weſtern inſtead of the Eaſtern edge, owing to a miſtake in the Almanac; ſo that the Eclipſe was now about its greateſt obſcuration. A remarkable protubera .ce on that part of the Moon's limb.
20	36	34	End of the Eclipſe. Obſerved with a 3½ feet Achromatic Teleſcope, magnifying 150 times.

Occul-

Occultations of Stars by the Moon. Observed with the same telescope, magnifying 150 times.

1772.		Apparent time.				
			h	′	″	
♄ Feb. 15.	☽ λ ♊		9	24	8	Im. good.
			10	21	16	Em. certain to 2″.
☉ April 12.	☽ ω ♌		11	35	17	Im. The * certainly began to lose of its light 2″ before it disappeared.
			12	1	35	Em.
♀ May 8.	☽ *		8	16	6	Im. Seen accidentally, as I was looking at ☽ with the magn. 100. The * was near ζ ♋, but less N. dec.
						Em. cloudy.
♄ May 9	☽ κ ♋		.	.	.	Not eclipsed.
♀ 15.	☽ α ♎	1ma.	11	56	50	Im. good. Night clear, and no air.
			13	0	23	Em. doubtful to 5″ or more.
		2da.	12	2	19	Im. good.
			13	8	57	Em. good.
☽ Aug. 17.	☽ ζ ♓	1ma.	10	57	25	Im. Night clear and still; but such undulation on ☽'s limb, that the imm. were doubtful.
			11	52	5	Em. good.
		2da.	10	58	12	Im. very doubtful.
			11	52	59	Em. very good.
☉ Sept. 6.	☽ ρ ♐	1ma.	8	41	41	Im. good.
						Em. Rain.
☽ 7.	☽ β ♑		13	9	21	Im. Hazy.
						Em. ☽ set.
♃ Oct. 8.	☽ *		9	7	46	Im. near Schikardus.
						Em. not seen.

Eclipſes of Jupiter's ſatellites. Obſerved with ſame teleſcope, magnifying 100 times.

1772.		App. time.			
		h	′	″	
♄ July	11.	11	22	34	Firſt ſat. im. Good.
♄	27.	9	38	2	Firſt ſat. im. Vapours.
☿	29.	10	29	59	Second ſat. im. Cloudy and doubtful.
☽ Aug.	3.	11	32	48	Firſt ſat. im. Good.
♄	22.	9	38	35	Third ſat. em. Hazy.
☉	23.	10	35	19	Second ſat. em. Doubtful. Flying clouds.
♀	28.	8	28	14	Firſt ſat. em. Very doubtful.
☉ Sept.	27.	10	53	35	Firſt ſat. em. Doubtful.
☉ Oct.	4	9	57	14	Third ſat. em. Good.
		12	50	41	Firſt ſat. em. Good.
♂	13	9	17	13,5	Firſt ſat. em. Good.
♂	20.	11	14	32	Firſt ſat. em. Doubtful.

Occultations of Jupiter's ſatellites, and tranſits over his diſc, and conjunctions of the ſatellites, and appearances of his belts. Obſerved with the ſame teleſcope, magnifying 150 times.

1772.		App. time.			
		h	′	″	
♀ Aug.	7.	10	44	40	Second ſat. on the limb of ♃, in its egreſs from his diſc.
♂	11.	10	55	50	Firſt ſat. in contact with ♃'s limb.
		11	1	40	——— Total ingreſs on the diſc. The ſhadow preceded the ſat. but very little.
☿	12	9	30	0	Appearance of ♃'s belts, as in Fig. I.
		10	45	0	Southern belt compleated.
♃	13	10	15	0	Appearance of ♃'s belts, as in Fig. II.
♀	21.	9	13	8	Conjunction of firſt and ſecond ſat.
♄	29.	9	2	0	Third ſat. ſeemingly in contact with the limb.
		9	13	0	——— Occult.
☉	30	9	45	0	Second ſat. partly covered by the limb.
		9	48	40	——— Occult.

1772.	App. time. h	′	″	
☽ Sept. 7.	9	38	0	Appearance of ♃, with the ſhadow of the fourth ſat. central. The ſat. itſelf not viſible. Fig. III.
	11	10	0	The Southern belt equally advanced with the ſhadow.
♄ 12	8	39	20	Appearance of ♃, and ſhadow of the firſt ſat. central, Fig. IV. The ſhadow much ſmaller than that of the fourth.
	9	24	30	Southern belt advanced half way.
	9	29	0	Sat. now off the diſc, and ſhadow ſtill on; about equidiſtant from the limb.
	9	40	46	Shadow going off, in contact with the limb.
♂ 15.	7	27	0	Appearance of ♃, as in Fig V.
	8	22	20	Southern belt compleat, and dark ſpot (*a*) in the large belt, central.
	8	27	20	Second ſat. on the limb.
	8	30	20	———— Total ingreſs on the diſc.
	10	41	0	Shadow advanced about $\frac{1}{3}$ on the diſc: not very viſible. preceded by another dark round ſpot, ſmaller; I know not what. Fig. VI.
☿ 16.	8	36	0	Shadow of the third ſat. about its own diameter from ♃'s limb. Well defined.
	9	19	0	Third ſat. quite off the diſc. The ſhadow was then advanced about $\frac{1}{3}$.
	9	33	0	Southern belt reduced to about $\frac{1}{5}$. The ſhadow advanced about $\frac{2}{5}$. Fig VII.
	9	40	0	Shadow central.
	11	2	30	Shadow advanced about $\frac{4}{5}$. Southern belt then ſcarce viſible on the Eaſt ſide.
	11	35	0	Shadow either gone off, or not viſible on the limb. Southern belt now come on $\frac{1}{4}$ or more. Vide Fig. VIII.
♄ 19.	8	43	30	Firſt ſat. ſeemingly in contact with ♃'s limb.
	8	52	0	———— Total ingreſs.
	9	7	40	Conjunction of ſecond and third ſat.
	9	32	0	Shadow of firſt ſat. half entered.
	9	35	30	———— totally entered.
	10	58	0	———— advanced about $\frac{2}{3}$.
	11	1	0	Firſt ſat. on the limb.
	11	4	30	———— external contact. Vide Fig. IX. Shadow very ſmall, but well defined.

1772.		App. time	
		h ′ ″	
☉ Sept.	27.	7 37 30	First sat. in contact.
		7 42 0	——— occult.
♃ Oct.	1.	8 9 26	Conjunction of third and fourth sat.
		8 14 45	Second sat. in contact.
		8 20 0 ::	——— occult.
☉	4	9 27 40	First sat. in contact.
		9 32 30	——— occult.
☽	5	8 29 45	First sat. in contact, just gone off the limb. The shadow at that time about $\frac{2}{3}$.
♂	20.	7 40 15	First sat. in contact.
		7 44 20	——— occult.

These last observations are not to be depended upon, within several seconds; and yet, if pursued regularly, might have their use. I was led into making them from what M. Messier transmitted to this Society, in a former paper, Vol. LIX. N° LXV. Indeed the hint he there gave, concerning the superiority of achromatic telescopes above all others for these purposes, induced me to procure one from Mr. Dollond, of his last construction, with a triple object-glass; which, I believe, is excellent in its kind: at least, it has fully answered the highest expectation I had formed of them.

The drawings shew how Jupiter appeared to me at different times. The broad belt across the middle, I cannot tell what to make of; sometimes it appears as a combination of several smaller ones; but usually clouded, or waved, in the middle, with its edges in general darkest; perhaps from their opposition to the contiguous parts, which are always much brighter than the rest of the planet. There have been two places in that belt particularly dark (especially that

 marked

marked *α*, Fig. V.), which I have ſeen latterly, whenever that ſide of ♃ has been turned towards us; though I did not perceive them Auguſt 12, when I took Fig. I. The Northern belt, I believe, is continued uniformly round the planet; the Southern one reaches little above half way. They are both of them darker, I think, than the general complexion of the largeſt. There is beyond each of theſe, a light part, not equally bright with thoſe zones near the middle; and from thence a gradual ſhading off to the poles themſelves; particularly to the North. The drawings are, as the objects appear in an aſtronomical teleſcope, and therefore inverted.

Chiſlehurſt,
November 10, 1772.

Francis Wollaſton.

THE

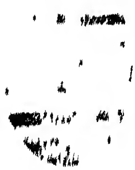
THE Aſtronomer Royal, who was preſent at the reading of this paper, expreſſed a wiſh that a particular account of the mechaniſm of the clock itſelf, might accompany the regiſter of its going; as no conſideration ought to be omitted which might ſerve to elucidate theſe matters.

My clock is in general of the plain kind, moving in braſs pivot-holes, beating dead ſeconds, with the common ſteel paletts; but of good workmanſhip, as being made by Mr. Holmes. The rod of the pendulum is of deal, as mentioned in a former paper; to which the ball itſelf (weighing about fourteen pounds) is ſcrewed faſt, there being a ſmaller weight underneath for a regulator. Its ſuſpenſion is ſomewhat particular. The ſpring A is not hung in a ſlit as is uſual; but is faſtened to a tranſverſe piece B, on which it reſts upon the ſides of the cock. The ſhoulders of this piece confine the pendulum from any lateral motion, as much as it would be in a ſlit; but it is at full liberty to hang perpendicularly under its point of ſuſpenſion, without any ſtrain on either edge of the ſpring above the other. The crutch is alſo of an unuſual make. The bottom of the ſtem, inſtead of receiving the crutch-pin, is turned ſideways; at right angles to itſelf, but parallel to the back-plate. This piece D reaches about an inch, and at the end has a ſmooth ſteady joint E (known to the workmen by the name of a ſocket and ſtud), whoſe axis lies horizontal. From that joint there is a return F, of equal length with the former piece; at the end of which is the crutch-pin G, nearly coinciding with the end of the ſtem.

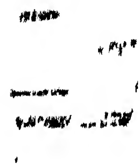
 This

This return together with the crutch-pin, if the pendulum were removed, would fall down as reprefented by the dotted lines *f*, *g*. The defign of this mechanifm is that any friction at the crutch-pin, which in the common conftruction is a fmall fliding up and down at every ofcillation, is hereby converted into a fmaller circular motion in a fmooth pivot-hole. Both thefe, I underftand, are contrivances of the late Mr. Henry Hindley of York: and may be feen in the regulator at Mr. Holmes's in the Strand, which is conftructed in the fame manner. The cock is faftened to the back-plate of the clock itfelf; but the clock-cafe is made ftronger than ufual, and is firmly fcrewed to the wall, independent on the floor.

Fig. I
Aug 12 1772
h 11 30 0

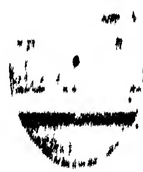


II
Aug 13
h 10 15 0

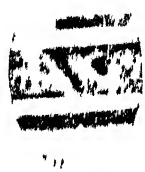


III.
Sept 7 h 9 38 0
Shadow of 4th Sat

IV
Sept 11 h 8 39 20
Shadow of 1st Sat

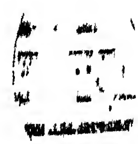


V
Sept 15 h 7 27 0



α

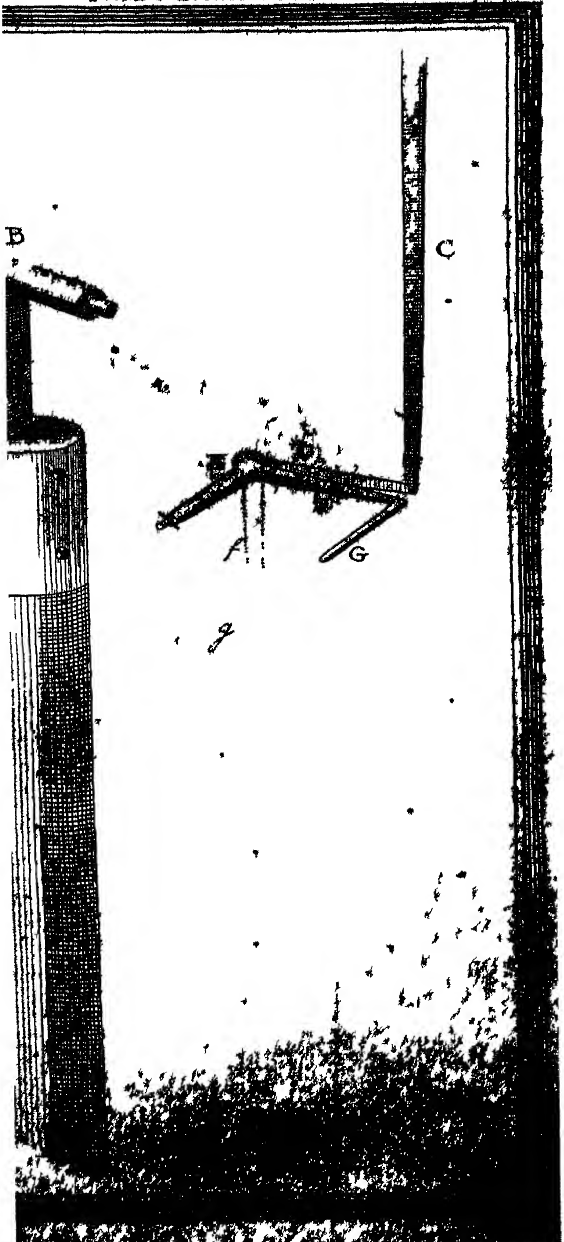
VI
Sept 15 h 10 41 20
Shadow of 2d Sat



VII.
Sept 16 h 9 00 0
Shadow of 1st Sat.

VIII.
Sept 16 h 11 35 0

IX
Sept 19 h 11 4 30
1st Sat. & its Shadow


B
C
A
G
g

XII. *A Letter from Dr.* Ducarel, *F. R. S. and F. S. A. to* William Watson, *M. D. F. R. S. upon the early Cultivation of Botany in* England; *and some Particulars about* John Tradescant, *a great Promoter of that Science, as well as Natural History, in the last Century, and Gardener to King* Charles I.

S I R,

Read Nov. 2, 1772.

YOUR love for Botany, and your great knowledge in that science, will, I hope, be a sufficient apology for laying before you some observations which have occurred to me on that subject. And I flatter myself that the following anecdotes will not be altogether unacceptable to you.

The sciences, we know, are subject to revolutions. But is it not a very extraordinary one that Botany, so useful to mankind, and so well known to the ancients, should, for some ages, abandon Europe, and remain almost unknown there till the sixteenth century; when it is supposed to have suddenly revived; and has since, by the industry of the moderns, been brought to the highest perfection?

The truth, however, is, that Botany returned into England long before this æra. It was brought back here by the Saxons; ſince whoſe time, I ſhall endeavour to ſhew, that it hath always flouriſhed, more or leſs, in this kingdom.

I found my opinion upon the authority of the four following Saxon manuſcripts.

Two in the Bodleian Library, viz.

(*a*) N° 4125. Herbarium Saxonicum.
(*b*) N° 5169. Liber Medicinalis MS. continens Virtutes Herbarum Saxonicè.

And two others in the Harleian Library, viz.

N° 5066. entitled, Herbarium Saxonicè.
N° 585. Tractatus qui ab Anglo-Saxonibus dicebatur LIBER MEDICINALIS: ſcil. L. Apuleii Madaurencis Libri de Virtutibus Herbarum, Verſio Anglo-Saxonica.

This Lucius Apuleius of Medaura was a famous Platonic philoſopher, who flouriſhed about A. D. 200.

From this time I have met with no MS. concerning Botany, till the thirteenth century, when (*c*) Biſhop Tanner mentions three MSS. on this ſubject, written by Gilebertus Legleus, ſive Anglicus, a phy-

(*a*) Cat. MSS. Angliæ, p. 185.
(*b*) Ibid. p. 562.
(*c*) Bibliotheca, p. 474.

ſician,

ſician, who flouriſhed in the year 1210, entitled,

1. De Virtutibus Herbarum, MS. Bodl. Digb. 75.
2. Gilberti Liber de Viribus & Medicinis Herbarum, Arborum, & Specierum, MS. olim Monaſt. Sion.
3. De Re Herbaria, Lib. I.

(*d*) The Biſhop likewiſe mentions one John Ardern, a famous ſurgeon, who lived at Newark in Nottinghamſhire from 1349 to 1370, as the author of a MS. (now extant in Sir Hans Sloane's library), entitled, Volumen Miſcellaneorum de Re Herbaria, Phyſica, & Chirurgica.

In the Aſhmolean Library are the following MSS. viz.

(*e*) (N° 7704.) entitled, A Treatiſe of Chirurgery, with an Herbal, &c. in Old Engliſh, 4to. 1438. And another,

(N° 7709.) called, An Herbary, &c. written alphabetically, according to the Latin names, in 1443. And

(N° 7537.) entitled, A Book of Plants and Animals, delineated in their natural colours on velom, Old Engliſh, A. D. 1504.

Mr. Ames, in his Typographical Antiquities, p. 470, informs us, that, in the year 1516, a folio, entitled, "The Greate Herball," was printed in

(*d*) Bibliotheca, p. 48.
(*e*) Cat. MsS. Angl. p. 341.

 South-

Southwark by Peter Treveris; and this, Sir, I believe, is the oldeſt Engliſh herbal now extant in print.

To come to later times. The ingenious Mr. Gough (in his Britiſh Topography, p. 61.) informs us, "That, before the year 1597, John Gerrard, " citizen and ſurgeon of London, ſeems to be the " firſt who cultivated a large phyſic garden, which " he had near his houſe in Holborn, where he " raiſed 1100 different plants and trees." (He might have add, that Gerrard had another phyſic garden in Old-ſtreet, containing a great variety of plants; a printed catalogue of which is to be found in the libraries of the curious). But Gerrard had a famous cotemporary, who greatly advanced that valuable ſcience, and of whom but little hath hitherto been ſaid by the modern biographers.

John Tradeſcant is the perſon meant. And I hope, Sir, that an attempt to revive the memory of this once eminent botaniſt and virtuoſo will not be diſpleaſing.

John Tradeſcant was, according to Anthony Wood, a Fleming, or a Dutchman. We are informed by Parkinſon, that he had travelled into moſt parts of Europe, and into Barbary; and, from ſome emblems remaining upon his monument in Lambeth church-yard, it plainly appears that he had viſited Greece, Egypt, and other Eaſtern countries.

In his travels, he is ſuppoſed to have collected not only plants and ſeeds, but moſt of thoſe curioſities of every ſort, which, after his death, were ſold by his ſon to the famous Elias Aſhmole, and depoſited in his Muſeum at Oxford.

When

When he firſt ſettled in this kingdom, cannot, at this diſtance of time, be aſcertained; perhaps it was towards the latter end of the reign of Queen Elizabeth, or the beginning of that of King James the Firſt. His print, engraven by Hollar before the year 1656, which repreſents him as a perſon very far advanced in years, ſeems to countenance this opinion.

He lived in a great houſe at South Lambeth, where there is reaſon to think his muſeum was frequently viſited by perſons of rank, who became benefactors thereto: among theſe were King Charles the Firſt (to whom he was gardener), Henrietta Maria his Queen, Archbiſhop Laud, George Duke of Buckingham, Robert and William Cecil, Earls of Saliſbury, and many other perſons of diſtinction (*f*).

John Tradeſcant may therefore be juſtly conſidered as the earlieſt collector (in this kingdom) of every thing that was curious in Natural Hiſtory, viz. minerals, birds, fiſhes, inſects, &c. &c. He had alſo a good collection of coins and medals of all ſorts, beſides a great variety of uncommon rarities. (*g*) A catalogue of theſe, publiſhed by his ſon, contains an enumeration of the many plants, ſhrubs, trees, &c. growing in his garden, which was pretty extenſive. Some of theſe plants are (as I am informed), if not totally extinct, at leaſt become very uncommon, even at this time. A liſt of ſome remarkable ones

(*f*) See a liſt of them at the end of *Muſeum Tradeſcantianum*, 12^mo^, London, 1656—where are Hollar's two prints of John Tradeſcant, the father and ſon.

(*g*) Ibid.

 introduced

introduced by him, is inserted below (*b*). And this able man, by his great industry, made it manifest (in the very infancy of Botany), that there is scarce

(*b*) From *Parkinson's Garden of pleasant Flowers*, printed in 1656.

1. "Pseudonarcissus aureus maximus flore pleno, sive roseus "Tradescanti.
   "The greatest double yellow bastard daffodil, or John "Tradescant's great rose daffodil.
   "This daffodil was primarily introduced by John Tradescant, and for its extreme beauty, may well be entitled "the glory of daffodils." . . . . Page 102.
2. "Moly Homericum, vel potius Theophrasti.
   "The greatest moly of Homer. . . . . 141.
3. "Moly Indicum, sive Caucason.
   "Indian moly. . . . . *ibid.*
   "Both the above molys are natives of Spain, Italy, and "Greece, and were procured from thence by John Tradescant, and flourished with him, in his garden at Can-"terbury," (should be South Lambeth).
4. "Ephemerum Virginianum Tradescanti.
   "John Tradescant's spider-wort of Virginia.
   "This spider-wort is of late knowledge, and for it the "Christian world is indebted unto that painful industri-"ous searcher and lover of all nature's varieties John "Tradescant." . . . . 152.
5. "Gladiolus Byzantinus.
   "Corn-flag of Constantinople.
   "With this species John Tradescant observed many acres of "ground in Barbary overspread. . . . . . . 190.
6. "Elleborus albus vulgaris.
   "White hellebore.
   "This groweth in many places in Germany, and also in "some parts of Russia, and in such plenty, that John "Tradescant observed quantity sufficient to load a good "ship with the roots. . . . . 346.
7. "Nardis montana tuberosa.
   "Knobbed mountain valerian.
   "Discovered in a botanic excursion by J. Tradescant. 388.

any

any plant extant in the known world, that will not, with proper care, thrive in this kingdom.

When his houſe at South Lambeth (then called Tradeſcant's Ark) came into Aſhmole's poſſeſſion, he added a noble room to it, and adorned the chimney with his arms, impaling thoſe of Sir William Dugdale (whoſe daughter was his third wife), where they remain to this day.

This houſe belongs at preſent to John Small, Eſq; who, about twelve years ago, purchaſed it of ſome of Aſhmole's deſcendants; and my houſe, once a part of Tradeſcant's, is adjoining thereto.

It were much to be wiſhed, that the lovers of Botany had viſited this once famous garden, before, or at leaſt in, the beginning of the preſent century. But this ſeems to have been totally neglected till the year 1749, when yourſelf, and the late Dr. Mitchel, favoured the Royal Society (*i*) with the only account now extant, of the remains of Tradeſcant's garden. In it, Sir, you ſeem to confine the extent thereof to

8. " John Tradeſcant introduced a new ſtrawberry, with " very large leaves, from Bruſſels; but, in the courſe " of ſeven years, could never ſee one berry completely " ripe. . . . . Page 528.

9. " John Tradeſcant procured a new and great variety of " plums from Turky, and other parts of the world. 575.

10. " The Argier, or Algier apricot. This, with many other " ſorts, John Tradeſcant brought with him, returning " from the Argier voyage, whither he went with the " fleet that was ſent againſt pirates, A° 1620." 579.

Thus far Parkinſon; but whether or no theſe plants bear his name at this period, I can no more pretend to aſſert, than that all the ſpecies therein mentioned are even now exiſting in our gardens.

(*i*) Philoſophical Tranſactions, Vol. XLVI. p. 160.

that

that now belonging to Mr. Small's houſe. I believe it was otherwiſe; and, on the account of the great number of plants, trees, &c. am inclined to think that Tradeſcant's garden extended much farther. Bounded on the Weſt by the road, on the Eaſt by a deep ditch (ſtill extant) it certainly extended a good way towards the North, and took in not only my orchard and garden, but alſo thoſe of two or three of my next neighbours; and ſome ancient mulberry trees, planted in a line towards the North, ſeem to confirm this conjecture.

When the death of John Tradeſcant happened, I have not been able to diſcover, no mention being made thereof in the Regiſter Book of Lambeth Church.

A ſingular monument (of which I herewith ſend you a drawing, Tab. IV. and V.) was erected, in the South Eaſt part of Lambeth church-yard, in 1662, by Heſter, the relict of John Tradeſcant the ſon, for himſelf, and the reſt of this family, which is long ſince extinct (*k*).

This once beautiful monument hath ſuffered ſo much by the weather, that no juſt idea can now, on inſpection, be formed of the North and South ſides. But this defect is happily ſupplied from two fine drawings, preſerved in Mr. Pepys's Library at Cambridge. We ſee

On the Eaſt ſide Tradeſcant's arms.
On the Weſt A hydra, and under it a ſkull.

(*k*) John the grandſon, buried 15th September 1652.
John the ſon, buried 25th April 1662.
Heſter, widow of John Tradeſcant, buried 6th April 1678.
From the Regiſter of Lambeth Church.

On the South — Broken columns, Corinthian capitals, &c. ſuppoſed to be ruins in Greece, or ſome other eaſtern countries.

On the North — A crocodile, ſhells, &c. and a view of ſome Egyptian buildings.

Various figures of trees, &c. in relievo adorn the four corners of this monument.

The following remarkable epitaph (preſerved at Oxford, and printed in Mr. Aubrey's Antiquities of Surrey, p. 11.), was intended for, but never placed upon, this monument.

Know, ſtranger, e'er thou paſs, beneath this ſtone
Lie John Tradeſcant, grandſire, father, ſon.
The laſt dy'd in his ſpring; the other two
Liv'd till they had travelled art and nature thro'.
As by their choice collections may appear,
Of what is rare in land, in ſeas, in air:
Whilſt they (as Homer's Iliad in a nut)
A world of wonders in one cloſet ſhut.
Theſe famous antiquarians that had been
Both gardiners to the Roſe and Lilly Queen,
Tranſplanted now themſelves, ſleep here; and when
Angels ſhall with their trumpets awaken men,
And fire ſhall purge the world, theſe hence ſhall riſe
And change their garden for a paradiſe.

Before I conclude, I muſt beg leave to add a liſt of the portraits of the Tradeſcant family, now in the Aſhmolean Muſeum. I cannot, however, conceive why both father and ſon are therein called Sir John, as it does not appear either of them were ever knighted. But ſo it is in the Oxford liſt communicated

cated to me, ſome time ſince, by the late worthy and learned Mr. William Huddesford, keeper of the Aſhmolean Muſeum.

1. Sir John Tradeſcant, ſenior. A three quarter piece, ornamented with fruit, flowers, and garden roots.
2. Ditto. After his deceaſe.
3. A ſmall three quarter piece. Water colours.
4. A large piece, of his wife, ſon, and daughter. Quarter length.
5. Sir John Tradeſcant, junior, in his garden. Half length, a ſpade in his hand.
6. Ditto, with his wife, in one piece. Half length.
7. Ditto, with his friend Zythepſa of Lambeth, a collection of ſhells, &c. upon a table before them. A large quarter piece, inſcribed Sir John Tradeſcant's ſecond wife, and ſon.

Theſe pictures have no date, nor painter's name, as I can yet find. They are eſteemed to be good portraits. Who the perſon was, called in the picture Zythepſa, I never could learn. He is painted as if entering the room, and Sir John is ſhaking him by the hand.

I have the honour to remain, with great eſteem,

S I R,

Your moſt faithful,

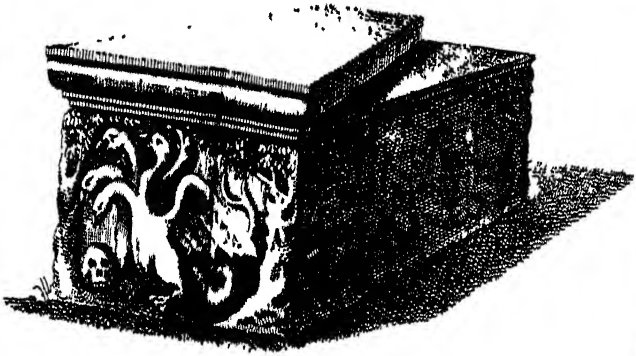
humble ſervant,

Doctors-Commons,
November 2, 1772.

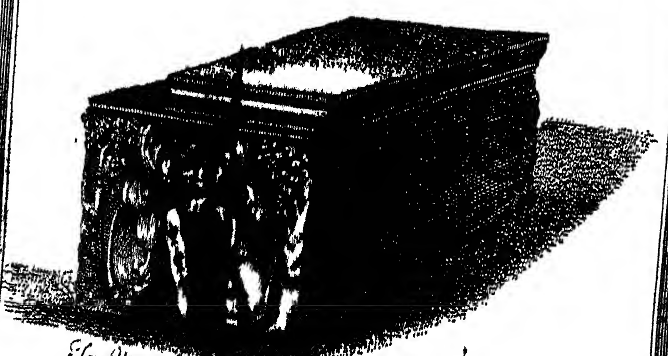
And. Coltee Ducarel.

XIII. *De*

From the West



From the East



Two Views of the Monument of JOHN TRADESCANT in the Church Yard of St. Mary Lambeth 1773.

B. T. Pouncy del. Basire sc.

From the SOUTH.

Philos. Trans. Vol. LXIII. Tab. V. p. 88.

In the PEPYSIAN Library.

From the

XIII. *De intenso Frigore Mensibus* Januario 1767 *ac* 1768, *&* Novembri 1770, *observato* Franequeræ, *Auctore* J. H. Van Swinden, *Philos. Profess. in Academia* Franeker. *Societ.* Harlemo-Hollandicæ *Socio.*

IN Transactionum Philosophicarum, tomis LVIII. & LIX. vidi nuperrime, illustrissimos horum Actorum Collectores observata quædam inseruisse circa acerbum frigus anni 1768: cum vero exinde liqueat hujusmodi annotationes usu non carere, in medium proferam quæ, vel propriis vel alienis observationibus, in patria institutis, ad notitiam meam pervenerunt.

Anno 1767, peritissimus telescopiorum catoptricorum artifex Vander Bild, thermometrum Fahrenheitianum observavit,

Jan. 6°.	h. 8	A. M. ad gr.	16
	h. 10	P. M.	2
7°.	h. 7½	A. M.	— 2
	12	. . . .	+ 12
	7	P. M.	5
	9	. . . .	7
8°.	7½	A. M.	12

Minima altitudo fuit ad minimum — 2, nam mercurius totus intra bulbum erat reconditus, adeoque ad minimum ad 2 gr. infra o, cum bulbus a o hac quantitate diſtet. Mercurius ab alio hic viſus fuit ad — 5, ut mihi memoratus Vander Bild narravit. Si vero comparationis tabellam formemus, habebimus,

	Franeq.	Amſt.	Leidæ.	Zwaneburgi.	Wel.
Die 7° Jan.	2	⊢ 4	5	6	— 3

Hæ autem obſervationes omnes, mane hora 7 aut 8 inſtitutæ fuerunt, excepta ea quam in pago Wel, prope Silvam Ducum inſtituit Rev. Palier: ibi therm. fuit,

h. 7. A. M. ad	— 2½	h. 2. ad	7¼	h. 9. P. M.	— 3
8. . . .	— 2	6.	o	10. . . .	o
9.	o	7.	— 1		
10.	1	8.	2		

(Videat. Harlem. Societ. Act. Belg. T. IX. p. 649. Partis III.). Notatum autem eſt maximum frigus A° 1740, in Hollandia fuiſſe — 2, ac — 4.

A° 1768. Obſervationes a meipſo inſtitutæ ſunt. Die Jan. 3, altitudo maxima, h. 12 ad gr. 13, minima gr. 7; h. P. M. 5. vento N. O. die 4, maxima, 20°, minima 12, vento O. ac Z. O. die 5, maxima 23°, minima 18, h. 11. P. M. vento Z. Z. O; ninxit hoc die. Die vero 6, hora 8¼ A. M. ther. 3½, Vento Z. O. cœlo ſereno: hora 9, ad 6°, h. 11½ ad 6°, h. 11, ad 12; h. 4. P. M. ad 10, hora 10 ad 11, cum valido vento orientali. Si iterum comparationem inſtituamus, habebimus,

Franeq.

Franeq	Amster.	Zwanenb.	Leidæ.	Wel.
		6 Die 3.		—3½ Die 3.
3½ Die 6.	7 Die 4.	7 Die 4.	4 Die 4.	—1 Die 4.
		8 6.	8 Die 6.	+7 Die 6.

Aliis in locis, uti Lipsiæ, Coloniæ, Regiomonti, &c. mercurium multum infra zero descendisse novellæ publicæ retulerunt. Sane si ad frigoris intensitatem & durationem adtendamus, hanc hiemem inter acerbissimas referemus: sed hæc hactenus, tantum enim maximos frigoris gradus adnotare volui.

Verum quæ mense Novembri 1770, hic observavi rariora sunt.

Die Nov. 17, gelu cum nive incepit, vento O. Die 18, h. 8, therm. 27½. Die 19, h. 8. A. M. 24°, hora 2, P. M. 28; h. 5, 19°: h. 10, 16°. vento O, ac O. Z. O. At die 20, h. 7½. therm. 9°. vento Z. Z. O. dein vero celeriter adscendit: nam hora 2, erat jam ad 25°. vento O. Z. O. cœlo sereno: h. 6, ad 32°, vento Z. Z. W. cum nive, hora 10, ad 32°. die 21, h. 10, ad 36°. Nescio an nostris in regionibus, tanti frigoris hoc mense observati dentur exempla. An imminens nix ejus causa exstitit? Si vero comparationem cum aliis locis instituamus, habebimus,

Franeq.	Amst.	Zwanenb.	Leidæ.	Hag.	Berol.
9	22	25	17	21	17¾

Die 17, 23. h. 11, P. M. 36.

Maxima mutatio hic fuit 27°. Magnam autem inter loca, haud multum ab invicem dissita, differentiam dari liquet, & sæpe fuit observatum: omnis

N 2 tamen

tamen a ſola thermometrorum ſitu vario repeti nequit.

Ad hanc frigoris intenſitatem accedit, celeremque hanc temperiei mutationem multum ſuperat, quod in vico Tooting, die 22 Nov. 1748, obſervavit Rev. Miles (Philoſ. Tranſact. N° 491. T. XLVI. p. 3.), ſc. Die 21 Nov. fuit therm. ad 7° gr. infra o, ſeu congelationis punctum. Die 22, h. $4\frac{1}{3}$, A. M. $14\frac{1}{2}$ infra o. h. 7, gr. 5 infra o, vento W. ac Z. W. Mane, hora 10, jam pluebat thermom. indicante 5 ſupra o, hora $8\frac{1}{4}$, P. M. therm. erat ad 12 ſupra o. Unde mutatio fuit gr. $26\frac{1}{2}$. Ait vero Cel. Maty (Jour. Britan. T. V. p. 124.), hoc therm. cum Fahrenheitiano convenire, niſi quod punctum o in puncto congelationis fuerit notatum. Unde in ſcala Fahrenheitiana erunt frigoris gradus hi.

22 Nov. 1748, h. $4\frac{1}{3}$ gr. $17\frac{1}{2}$. h. 7. gr. 27. h. 10, gr. 37. cum pluvia, h. $8\frac{1}{4}$, P. M. gr. 42. Frigus ergo minus eo quod hic obſervavi.

Franeq. 13 Nov. 1772. J. H. Van Swinden.

P. S. Anno 1771, die Jan. 13, vidi therm. ad 8 gr. die Feb. 11, ad gr. 13. die Jan. 19, 1772, ad 11°. Verum de his alia opportunitate latius.

XIV. An

XIV. *An Inquiry into the Quantity and Direction of the proper Motion of* Arcturus; *with ſome Remarks on the Diminution of the Obliquity of the Ecliptic: By* Thomas Hornſby, *M. A.* Savilian *Profeſſor of Aſtronomy in the Univerſity of* Oxford, *and F. R. S.*

Read Dec. 24, 1772.

AS an accurate knowledge of the *poſition of the fixed ſtars* is of the greateſt importance, being the baſis and foundation of aſtronomy, it is no wonder that the aſtronomers of different nations have given great attention to this matter. By comparing *antient* with the beſt *modern obſervations*, it appears that ſome of the fixed ſtars have a *proper motion*, independent of any motion hitherto known in our own ſyſtem; or that, in other words, the *angular diſtances of the fixed ſtars have not always continued the ſame*, and in ſome of them the alteration is ſo very conſiderable as to be eaſily perceived in the courſe of *a few years*, with inſtruments accurately made, and nicely adjuſted. Of all the ſtars viſible in our hemiſphere, the *variation* in the place of *Arcturus is the moſt remarkable*, and ſuch as cannot poſſibly be attributed to the uncertainty of obſervation. It has accordingly been taken no-

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tice of by many aſtronomers: in particular, Dr. *Halley* mentions it in N° 355 of the Philoſophical Tranſactions: Mr. *Caſſini*, in the Memoirs of the Academy of Sciences for 1738, p. 231, has ſhewn, that there is a variation of *five minutes* in the latitude of that ſtar between his own time and that of Tycho, in an interval of a *century and a half*: and M. le *Monnier*, in the Memoirs of the Academy of Sciences for 1767, p. 417, proves, that the *latitude* of Arcturus varies at the rate of *two ſeconds* every *year*; and that the *longitude* decreaſes at the rate of 60″ in a hundred years *. But as an *inquiry* as well into the true *quantity* as into the *direction* of this motion has not hitherto been made public, I propoſe to give ſome account of my own obſervations made expreſsly with this view in the years 1767 and 1768, with a tranſit inſtrument of 44 inches, and a moveable mural quadrant of 33 inches, both conſtructed by Mr. Bird, and of the concluſions reſulting from a compariſon between them and ſome obſervations made by Mr. Flamſteed in 1690.

It may perhaps be objected, that the differences of right aſcenſion, as determined by Mr. Flamſteed's mural inſtrument, are not to be depended upon from the very nature of his inſtrument. Mr. Flamſteed was himſelf too good an obſerver not to be aware of this; and accordingly in the Prolegomena to the third volume of the *Hiſtoria Cæleſtis*, p. 132, he informs us in what manner he determined the error of

* See alſo the Memoirs of the Academy of Sciences for 1769, p 21. See alſo Aſtronomiæ Fundamenta, by the Abbé de la Caille; who, in reducing his obſervations of Arcturus, ſuppoſes the annual motion of declination in that ſtar = 19″,0. p. 169, and 187.

the

the plane at different diſtances from the zenith. By diſtributing theſe errors in the beſt manner I could, I am of opinion, that the error of the plane of his inſtrument may be ſuppoſed to decreaſe uniformly at the rate of half a ſecond in time for every degree of zenith diſtance from 28° to 60°, the error being 39″ at the former, and 23″ at the latter, by which quantity ſtars paſſed the horary wire, in his inſtrument, before they came to the true meridian. It ſhould ſeem alſo that the error continued nearly the ſame from 60 to 75 degrees, being at the latter only 22″: but that it decreaſed irregularly from 75° to 85°, viz. 1″ in time for each degree from 75° to 80°, and 0″,4 for each degree from 80° to 85 degrees. The mural arc was fixed upon a ſtone pier, the ſouthern part of which was found to ſettle yearly, from whence the error of the line of collimation to the ſouth neceſſarily became every ſucceſſive year greater and greater. As Mr. Flamſteed ſeems not to have had any method of adjuſting his inſtrument by a plumb-line, theſe errors muſt have been irregular at different ſeaſons of the ſame year, and were perhaps never truly determined. But as the obſervations here referred to were made on the ſame day, and within the compaſs of an hour, they are probably not affected with this latter error. We are at preſent concerned with the difference of two zenith diſtances, and not with the abſolute quantity of thoſe zenith diſtances. The concluſions may indeed be affected with an error in the diviſions; and from the examination which I have been able to make, I am of opinion that the arc of Mr. Flamſteed's inſtrument was not of the proper quantity; and that, though the obſervations generally

generally erred in defect, they in some parts erred in excess.

On the 14th of February, 1690, Mr. Flamsteed observed, that a small star, of the seventh or eighth magnitude, whose place is not determined in the British catalogue, and which star was named by him *Infra Arcturum*, preceded Arcturus three seconds in time, or 3″,3, when an allowance is made for the error of the plane of the instrument = 0′ 42″, 6, and was 26′ 30″ to the south of Arcturus *. By a mean of eight observations made at Oxford, on and near June the 10th, 1767, with the transit instrument, and with a refracting telescope of eight feet, furnished with a micrometer; the difference of right ascension was 1′, 8″,75 of a degree, the star following Arcturus; and by a mean of three observations, the extremes differing only 3″, the small star was 23′ 55″,0 to the south of Arcturus.

The right ascension of Arcturus and the small star being nearly the same, the change in declination ought to be so likewise. But, from the observed difference in declination, the right ascension of the two stars must vary unequally, though with a very small difference. Accordingly it appears from computation (in which the annual precession is supposed = 50″,35, the obliquity of the ecliptic at the middle of the interval of the time = 23° 28′ 30″, and the right ascensions and declinations of the two stars taken at a mean between the times of observation) that the variation of Arcturus in right ascension was 3270″,6, and of the small star 3277″,6 in

* This is the only observation of that star made by Mr. Flamsteed.

77,287 years. Therefore the right aſcenſion of Arcturus alters leſs than that of the ſtar; and conſequently Arcturus ſhould in 1767 have followed the ſtar by 42″,6. But the ſtar was obſerved to follow Arcturus by 1′ 8″,75. The right aſcenſion therefore of Arcturus has increaſed leſs than that of the ſtar, or Arcturus has moved weſtward 1′ 51″,35 in 77,287 years; and has gone ſouthward 2′ 35″ in the ſame time, ſuppoſing the ſmall ſtar not to have moved, which is highly probable.

On the ſame day the difference of right aſcenſion in time between the ſtar η Bootis and Arcturus was 21′ 32″ of mean ſolar time, = 5° 24′ 02″,2, when a proper allowance is made for the going of the clock, and for the error of the plane of the inſtrument; and the difference of declination was 50′ 45″,6, when an allowance is made for refraction. On the 24th, 26th, and 29th of May, and the 9th of June, of the year 1768, I determined the difference in right aſcenſion to be 21′ 27″ of ſidereal time by the two former obſervations, and 21′ 26¾″ by the two latter, the difference in declination being 49′ 48″,7, by a mean of the obſervations in May, the extremes differing only four ſeconds. It appears from computation, that between the times of obſervation the variation of η Bootis in right aſcenſion was 3371″,7, and 1417″,3, in declination; of Arcturus 3311″,7 in right aſcenſion, and 1347″,9 in declination: The difference of variation in right aſcenſion is 1′ 0″, and of declination 1′ 9″,4; by the former the difference in right aſcenſion was diminiſhed, and in declination increaſed by the latter, agreeably to the places of the two ſtars. The difference in right aſcenſion

 there-

therefore in 1768, if neither of the ſtars had moved, ſhould have been = 5° 23′ 02″,2, and 51′ 55″ in declination; but they were obſerved to be 5° 21′ 43″,4, and 49′ 48″,7. Arcturus therefore by this obſervation has in 78,257 years gone 1′ 18″,8 to the weſt, and 2′ 06″,3 to the ſouth, ſuppoſing η Bootis not to have any proper motion.

On the 5th of April, 1691, the difference in right aſcenſion between η Bootis and Arcturus was 21′ 33″ of mean ſolar time, = 5° 24′ 14″,0; and the difference of declination 50′ 45″,6, as in the preceding example. The difference of variation in right aſcenſion is 59″,1, and in declination 1′ 8″,4. The difference of right aſcenſion therefore at the latter end of May, 1768, ſhould have been 5° 23′ 14″,9, and 51′ 54″,0 in declination; but, according to obſervation, they were 5° 21′ 43″,4, and 49′ 48″,7. Arcturus therefore, according to this obſervation, has moved 1′ 31″,5 to the weſt, and 2′ 05″,3 to the ſouth in 77,120 years.

On the 4th of May, 1691, the difference of right aſcenſion between η Bootis and Arcturus was 21′ 33″ of mean ſolar time, = 5° 24′ 14″,3 when allowance is made for the going of the clock and the error of the plane of the inſtrument, and the difference of declination on the 3d of May = 50′ 50″,6. According to computation, thoſe differences ſhould have been 5° 23′ 15″,2 and 51′ 59″,0 reſpectively; but they were obſerved to be 5° 21′ 43″,4, and 49′ 48″,7. Arcturus therefore in 77,071 years has moved 1′ 31″,8 weſtward, and 2′ 10″,3 ſouthward. N. B. The zenith-diſtance of Arcturus,

as

as determined by Mr. Flamſteed, on the 4th of May, is manifeſtly erroneous.

On the 27th of May, 1692, η Bootis preceded Arcturus in right aſcenſion by 21′ 32″,5 of mean ſolar time, = 5° 24′ 10″,1, the difference of declination being 50′ 50″,6. In 75,978 years the difference of right aſcenſion ſhould have been 5° 23′ 11″,8 and 51′ 58″,0 in declination; but thoſe differences were obſerved to be 5° 21′ 43″,4 and 49′48″,7. Arcturus therefore has moved 1′28″,4 weſtward, and 2′ 09″,3 ſouthward.

On the 27th of May, 1692, Arcturus preceded π Bootis in right aſcenſion by 24′ 35″,5 of mean ſolar time, = 6° 9′ 32″,2, when an allowance is made for the going of the clock and the error of the plane of the inſtrument, the difference of declination being 3° 2′ 28″,9. On the 24th and 26th of May, and 5th of June, 1768, the difference of right aſcenſion between the ſame ſtars obſerved at Oxford was 24′ 44″,58 of ſidereal time, = 6° 11′ 9″,1, the difference of declination being 2° 58′ 24″,2. In 1768, the difference of right aſcenſion ſhould have been 2″,7 greater, = 6° 9′ 34″,9; and the difference of declination 1′ 31″,7 leſs, = 3° 0′ 57″,2. But they were obſerved to be 6° 11′ 9″,1, and 2° 58′ 24″,2. Arcturus therefore in 75,978 years has, by a compariſon with this ſtar, moved 1′ 34″,2 weſtward, and 2′ 33″,0 ſouthward.

Again, the difference of declination between Arcturus and π Bootis was obſerved to be 3° 2′ 33″,9 on the 14th of February, 1690, when the difference of right aſcenſion between theſe two ſtars was not obſerved by Mr. Flamſteed. It appears by computation,

tation, that the difference of variation in declination between the times of obfervation was 1′ 34″,5, by which quantity the difference of declination was diminifhed, and fhould therefore in 1768 have been 3° 0′ 59″,4. But it was 2° 58′ 24″,2 by actual obfervation. Arcturus therefore by this obfervation has moved fouthward 2′ 35″,2 in 78,255 years.

By the foregoing comparifons Arcturus appears to have moved as in the following table.

	years.	Weftwd. ′ ″	Southwd. ′ ″
By the fmall ftar Feb. 14, 1690, in	77,237	1 51,35	2 35,0
η Bootis Feb. 14, 1690, in	78,257	1 18,8	2 6,3
η Bootis Apr. 5, 1691, in	77,120	1 31,5	2 5,3
η Bootis May 4, 1691, in	77,071	1 31,8	2 10,3
η Bootis May 27, 1692, in	75,978	1 28,4	2 9,3
By π Bootis May 27, 1692, in	75,978	1 34,2	2 33,0
π Bootis Feb. 14, 1690, in	78,257	not obf.	2 35,2

As the quantity of the motion of Arcturus fouthward in declination, as deduced from a comparifon with η Bootis, differs confiderably from the quantities given by the fmall ftar and π Bootis, which agree very nearly together, I have compared η Bootis with fome of the neighbouring ftars, as that ftar, though of the third magnitude only, may have a fmall motion of its own.

On the 14th of February, 1690, the difference of declination between η and π Bootis was obferved by Mr. Flamfteed to be = 2°, 11′ 47″,8. By computation, that difference in 1768 fhould have been 2′ 43″,9 lefs, = 2° 9′ 3″,9: but it was actually obferved

observed to be 2° 8′ 34″,3 only. The star η Bootis therefore appears by this comparison to have moved southward 29″,6 in 78,257 years.

On the 27th of May, 1692, η Bootis was observed by Mr. Flamsteed to be 2° 11′ 37″,8 to the north of π Bootis, which quantity should by computation be 2′ 39″,1 less in 1768, or 2° 8′ 58″,7. But it was found to be 2° 8′ 34″,3. The star η therefore appears to have moved southward 0′ 24″,4 in 75,978 years.

On the 25th of April, 1693, η Bootis was observed to be 40′ 20″,8 to the south of *f* Bootis, a star of the 6th magnitude; and by myself that difference was observed to be 42′ 37″,5, by taking the mean of two observations on the 24th and 26th of May, 1768, differing only 4″,7. According to computation, the variation of η Bootis in declination during the interval of the two observations was 1359″,3, and of *f* Bootis 1256″,0; and therefore the difference of variation in declination was 1′ 43″,3, by which the distance of the stars was increased. The difference in declination therefore in 1768, if neither of the stars moved, should have been 0° 42′ 04″,1; but it was observed to be 33″,4 greater, by which quantity therefore η Bootis must have moved southward in 75,052 years.

By reducing all the foregoing deductions to 78 years, Arcturus appears to have moved,

Westw.

		Westw.	Southw.
		′ ″	
By the small Star,	Feb. 14, 1690	1 52,380	2 36,43
η Bootis	Feb. 14, 1690	1 18,541	2 5,88
η Bootis	Apr. 5, 1691	1 32,557	2 6,75
η Bootis	May 4, 1691	1 32,906	2 11,87
η Bootis	May 27, 1692	1 30,752	2 12,74
By π Bootis	May 27, 1692	1 36,707	2 37,07
π Bootis	Feb. 14, 1690	not observ.	2 34,69

But the Star η Bootis appears also to have moved Southward.

		′ ″
By π Bootis	Feb. 14, 1690	0 29,503
π Bootis	May 27, 1692	0 25,049
By *f* Bootis	Apr. 5, 1693	0 34,712
By a mean		0 29,755

As Arcturus appears to have moved southward of η Bootis 2′ 9″,31, by taking a mean of the four quantities resulting from the comparisons with that star; and as η Bootis has also moved southward of some of the neighbouring small stars by 29″,755 in the same time, Arcturus upon the whole has moved 2′ 39″06 to the south, by the comparisons with η Bootis only; and therefore, by taking a mean of all the results, Arcturus has altered its right ascension less than the neighbouring stars by 1′ 33″,97 in 78 years, in which time it has also moved 2′ 36″,81 to the south of the same stars.

In order to see how far the motion of right ascension is to be depended upon, which is deduced from the above comparisons, I have selected and computed the following observations, made at Shirburn castle

castle with a transit instrument of $5\frac{1}{4}$ feet, placed exactly in the plane of the meridian, and consequently more to be relied upon than those made with a mural instrument.

By a mean of five observations, made on the 7th, 12th, 23d, 24th, and 31st of May, 1741, O. S. the difference in right ascension between η Bootis and Arcturus was $5^\circ\ 22'\ 38'',9$, the extremes differing only $4'',4$ of a degree. The difference in the variation of right ascension to the end of May, 1768, is $20'',5$, by which the ascensional difference is diminished. It should therefore have been $5^\circ\ 22'\ 18'',4$; but it was observed to be $5^\circ\ 21'\ 43'',4$. Therefore in 27 years Arcturus has moved westward $35'',0$.

On the 16th and 20th of May, 1744, the difference in right ascension between η Bootis and Arcturus was $5^\circ\ 22'\ 30'',0$ by each of the observations, which difference should have been, supposing neither of the stars to have any proper motion, $5^\circ\ 22'\ 11'',7$ in May, 1768. But it was found to be $28'',3$ less; by so much therefore had Arcturus moved westward in 24 years.

On the 24th of May, and 8th of June, 1746, the difference in right ascension between the same stars was $5^\circ\ 22'\ 26'',2$, by taking a mean of the two observations; that difference should have been $5^\circ\ 22'\ 09'',5$ in 1768. But it was observed $= 5^\circ\ 21'\ 43'',4$. Arcturus therefore in 22 years has moved $26'',1$ to the west.

Lastly; on the 16th of April, and 27th and 28th of May, 1747, the difference in right ascension between η Bootis and Arcturus, by taking a mean of the three observations, was $5^\circ\ 22'\ 25'',0$. By computation

putation the variation in the difference of right afcenfion was 16″,0, by which the afcenfional difference fhould have been diminifhed, and = 5° 22′ 09″,0. But by obfervation it was found = 5° 21′ 43″,4: Arcturus therefore by this laft obfervation appears to have gone 25,6,″ weftward.

By the obfervations therefore at Shirburn Caftle Arcturus appears to have gone weftward as in the following table; in the laft column of which are contained the quantities refulting from the obfervations of each year, reduced to 78 years.

	″	′ ″
1741	0 35,0	1 41,11
1744	0 28,3	1 31,97
1746	0 26,1	1 32,59
1747	0 25,6	1 34,90
	Mean	1 35,14

The mean of all the obfervations, when reduced to an interval of 78 years, is 1′ 35″,14, which differs only 1″,17 from the mean of the other comparifons.

As then the proper motions of Arcturus weftward in right afcenfion = 1′ 33″,974, and 2′ 36″, 81 in declination fouthward, feem well eftablifhed, the real motion of Arcturus is inclined in an angle of 30° 56′ to the weft of the meridian or horary circle, and to be in that direction 3′ 2″,81 in 78 years, or at the rate of 2″,343 in a year. As this direction of its motion is nearly perpendicular to the plane of the ecliptic, the latitude of Arcturus muft diminifh yearly almoft in the fame proportion; and its longitude will alter lefs than that of other ftars, though not fo confiderably as its right afcenfion.

The

The proper motion of Arcturus then, in right aſcenſion Weſtward, being 1″,205, and in declination 2″,005, its annual preceſſion in right aſcenſion is 41″,108, and in declination 19″,133; and the true right aſcenſion of Arcturus, on Jan. 1, 1773, is 211° 19′ 47″,4, and declination North 20° 22′ 23″,3.

As none of the other principal ſtars have been found to have a motion ſo conſiderable as this, though many of the ſtars of the firſt magnitude, as for inſtance, Sirius, Procyon, α Aquilæ, α Orionis, as alſo β Aquilæ of inferior magnitude, do really vary their poſitions (and perhaps all of the firſt order will hereafter be found to have a proper motion), we may, I think, fairly conclude that Arcturus is the neareſt ſtar to our ſyſtem, viſible in this hemiſphere. If therefore the annual parallax of the fixed ſtars can ever be diſcovered, that is, if the diameter of the annual orbit bear a ſenſible proportion to the diſtance of the neareſt fixed ſtar, it is moſt likely to be diſcovered from the obſervations of Arcturus. The ſyſtem of the world, conſidered in an enlarged ſenſe, and agreeable to the idea we may entertain of an all-powerful benevolent Creator, may be taken to occupy the whole abyſs of ſpace, and to conſiſt of an aſſemblage of bodies, having different magnitudes, and emitting various degrees and modifications of light. The apparent change of ſituation viſible from the planet which we inhabit, and which revolves round one of the great bodies conſtituting a part of the general ſyſtem, as a center, may be owing either to the motion of our own ſyſtem in abſolute ſpace, or, if our ſyſtem ſhould be at reſt, to a real motion in

the ſtars themſelves: from whence the angular diſtances of the ſtars muſt vary in proportion to the velocity of thoſe motions, or to the direction of thoſe motions with reſpect to ourſelves. I have reaſon, at preſent, to believe that a ſmall motion may be diſcovered in the ſtar *o* Ceti, and perhaps in other ſtars that vary in degrees of brightneſs, which the diligence of future aſtronomers will diſcover, and perhaps in leſs time than at firſt ſight might ſeem neceſſary, when we conſider the ſeveral improvements which have of late been made in the methods of obſerving the heavenly bodies.

As the motion of Arcturus in declination (the quantity of which we have thus endeavoured to aſcertain) has been often acknowledged, it is matter of wonder that ſome aſtronomers, by comparing either the altitude or zenith diſtance of the Sun's limb with Arcturus, without previouſly ſettling the quantity of that ſtar's motion in declination, or at leaſt doing it indirectly, ſhould endeavour to determine whether the obliquity of the ecliptic hath remained conſtant, or ſtill continues to diminiſh, as it ſhould ſeem to have done for many centuries paſt, from the obſervations of ſucceſſive aſtronomers. Mr. Caſſini, and Mr. le Monnier, have both practiſed this method, and are of opinion, that the obliquity of the ecliptic hath not altered; or, if it hath altered, that the quantity of its alteration is not near ſo conſiderable as hath been imagined by ſome celebrated aſtronomers. By obſerving for ſeveral days before and after the ſolſtice the altitude or zenith diſtance of the Sun's limb, and that of a ſtar ſituated near the ſame parallel, the differences to be remarked in proceſs of time

time in the diſtances of the Sun from that ſtar (the motion of the ſtar in declination being allowed for during that interval of time), will be the quantity by which the Sun will have approached to or have receded from the ſtar. If the ſtar were abſolutely a fixed point, and the obſervations ſufficiently numerous, that, by taking a mean, the neceſſary and unavoidable errors in obſervation might either be conſiderably diminiſhed or almoſt annihilated, the method might be practiſed to great advantage. But as the ſtar (Arcturus) had a proper motion, and its apparent place was continually varying from the effect of the nutation of the earth's axis; as the limb of the Sun was ſometimes approaching to, and ſometimes receding from, the ſtar, by a kind of libratory motion from the effect of the nutation; alſo and as the obliquity of the ecliptic itſelf was, in all probability, continually diminiſhing; from a combination and as it were involution of theſe motions no certain concluſion could be drawn, ſince, in the ſpace of a few years, the apparent obliquity may be the ſame, and yet the mean obliquity may have diminiſhed, or perhaps, in the ſpace of a few years, the obliquity may appear to have increaſed, when it may really have become leſs. Whereas, by reducing the obſervations to their mean poſition, and by aſſigning to each known cauſe its proper and allowed effect, a regularity and uniformity muſt neceſſarily take place, as far at leaſt as is conſiſtent with the unavoidable errors in obſerving.

Mr. Caſſini, in the Memoirs of the Academy of Sciences for 1767, acquaints us, that, in 1748, the apparent diſtance of Arcturus from the upper limb

 of

of the Sun, at the time of the ſolſtice, was the ſame as in 1766.

	°	′	″	‴
In 1748. Diſtance of Arcturus from the ſolſtitial limb of the Sun	3	13	36	40
Altitude of Arcturus	61	41	17	0
Therefore the apparent ſolſtitial altitude	64	55	13	40
In 1766. Diſtance of Arcturus from the ſolſtitial limb of the Sun	3	19	32	0
Altitude of Arcturus	61	35	42	0
Therefore the apparent ſolſtitial altitude	64	55	14	0

The ſame aſtronomer has, in the Memoirs for 1759, p. 325, communicated the following concluſions.

		Diſt. of the ſtar from ☉'s limb.			Reduction.			Solſtitial diſtance.		
1763.		°	′	″		′	″	°	′	″
June	14.	3	7	29	+	11	1	3	18	30
	15.	3	10	16	+	8	13	3	18	29
	25.	3	15	40	+	2	48	3	18	28
July	1.	2	59	1	+	19	22	3	18	23
	2.	2	54	55	+	23	33	3	18	28
	3.	2	50	18	+	28	8	3	18	26
							Mean	3	18	27

Mr. le Monnier, in the Memoirs for 1762, p. 269, has publiſhed the following diſtances of Arcturus from the limb of the Sun, reduced to the ſolſtitial point, with a view to obtain differences in the apparent obliquity of the ecliptic: and, from the obſervations

vations made with the gnomon of St. Sulpice, and communicated by Mr. le Monnier, in the ſame volume, it ſhould ſeem that that aſtronomer is of opinion, that the obliquity of the ecliptic hath no other variation than what the nutation of the earth's axis will occaſion; and that therefore we muſt either abandon the abſolute diminution of the ecliptic, or at leaſt ſuppoſe it extremely ſmall, ſince, in the ſpace of eighteen years, it hath not produced a ſenſible alteration.

	°	′	″	
1738.	3	10	15	
1740.	3	11	5	
1742.	3	11	48	
1763.	3	18	40	with the mural quadrant of 5 feet.
	3	18	35	with the large mural inſtrument.

As the reſult of the obſervations only, and not the obſervations themſelves, are communicated, I have only to obſerve, that there is a very conſiderable difference between the concluſions of the two aſtronomers for the ſame year 1763, and, at the ſame time, to declare my ſuſpicion, that if the *apparent* (for ſuch I apprehend them to be) were reduced to the *mean* diſtances, they would probably afford a confirmation of the diminution of the ecliptic. For the following obſervations of the Sun's zenith diſtance, made at Shirburn Caſtle, near the ſummer ſolſtices of the years 1743, 1746, 1748, and 1766, and of Arcturus in the years 1743, 1746, and 1766, when reduced to their mean ſtate at the ſolſtice, do not confirm the aſſertion of Mr. Caſſini, but are an evident and

and absolute proof that the obliquity of the ecliptic has sensibly diminished during an interval of 23, and even of 18 years.

The observations of 1743 were made with a mural quadrant of five French feet, constructed by the late Mr. Sisson: but as the linear divisions were found to be somewhat less accurate than was expected, and as the body of the quadrant was not framed with proper strength and solidity, Mr. Bird was employed in the summer of the year 1745, by the Earl of Macclesfield, (the body of the instrument having been strengthened by screwing a large and broad plate of brass upon the cross-bars), to put a set of points upon the limb between the 90 and 96 arches of linear divisions. By these operations the line of collimation was found to have varied, and to be $= 6',3$, by which the zenith distances were given too small, by the positive divisions, from the end of 1746 to the end of June 1751, when Mr. Bird bisected the spaces between the points which he had formerly added in 1745. But after the year 1751, the error of the line of collimation was $= 2'',6$, as appears from observations of γ Persei, β and γ Draconis, by which the zenith distances are also given too small; and in that state the instrument continued to the year 1767, when a new set of wires was put into the telescope, and the line of collimation thereby altered. The error of the line of collimation from 1743 to 1745 cannot directly be ascertained, for want of zenith observations; but, from some indirect methods, it should seem that the error was as nearly as possible $=2''$, to be added to the observed zenith distances.

1743.

	Obferved zenith diftance.	Baro-	Thermo-meter.	Refrac-tion.	Sun's femi-diameter.	Dift. from folftice.	Obfervations reduced to folftice.
1743.				″ +			
June 7.	Up. L. 28 37 39	29,54	62	30.2	+ 15 48,1	43 5,7	28 10 51,6
8.	Lo. L 29 3 37	29,36	60	30,8	15 48	57 22,3	28 10 57,5
9.	Up. L. 28 26 44,2	29 46	59½	30	+ 15 47,9	32 3,3	28 10 58,8
12.	Up. L. 28 13 14,5	29,72	61½	29,9	+ 15 47,6	18 31,6	28 11 0 4
18.	Up L. 27 57 9	29.81	67	29,4	+ 15 47,3	2 30	28 10 55,7
19.	Lo. L. 28 27 28,5	29,54	67½	29,8	— 15 47,2	1 16,1	28 10 55
21.	Up. L. 27 54 41,8	29,66	66	29,1	+ 15 47,1	0 2,8	28 10 55,2
22.	Lo. L. 28 26 22,2	29,50	63	29,8	— 15 47	0 3.4	28 11 1,6
23.	Up. L. 27 55 10,2	29,59	61	29,5	+ 15 47	0 28,6	28 10 58,1
24.	Lo. L. 28 27 35,5	29,59	58	30,4	— 15 46,9	1 18,8	28 11 0,2
25.	Up. L. 27 57 15,5	29,39	58	29,5	+ 15 46,9	2 33,7	28 10 53 2
27.	Lo. L. 28 32 34	29,57	58	30,5	— 15 46,9	6 17,9	28 10 59,7

Mean . . 28 10 58,2
Sun's parallax — 4,1

28 10 54,1
Nutation + 0,7

28 11 0,8
Line of collimation . . . . . + 2

Mean folftitial zenith diftance, 1743, 11 2.8

	Obferved zenith diftance.	Baro-	Thermo-meter.	Refrac-tion.	Sun's femi-diameter.	Dift. from folftice.	Obfervations reduced to folftice.
1746.						°	
May 31.	Lo. L. 29 57 50,5	29,48	78	30,5	— 15 48,8	1 31 41,9	28 10 50,3
June 5.	Up. L. 28 48 24,8	29,62½	64 —	30,4	+ 15 48,3	53 50	28 10 53,5
6.	Lo. L. 29 13 36,3	29,62	65	30,8	— 15 48,2	47 26,3	28 10 52,6
7.	Up. L. 28 35 58 9	29,53	71 +	29,4	+ 15 48,1	41 24,3	28 10 52,1
10.	Lo. L. 28 52 1,8	29,28	67	29,8	— 15 47,8	25 49 8	28 10 54
16.	Up. L. 28 0 14,4	28,69	59½	28,7	+ 15 47,3	5 34,6	28 10 55,8
19.	Lo. L. 28 27 5,1	29,60	58½	30,3	— 15 47,2	0 59,8	28 10 48,4
	Up. L. 27 54 45	29,75	63½	29,4	+ 15 47	0 8	28 10 53,4
	Lo. L. 28 26 47,4	29 8:	64	30,2	— 15 47	0 40,4	28 10 50,2
	Up. L. 27 59 23,4	29,63	69	29	+ 15 46,9	4 45 9	28 10 53,4
27.	Lo. L. 28 33 6,6	29 63	74½	29 2	— 15 46,9	6 57,1	28 10 51,8
28.	Up. L. 28 4 11,8	29,50	65½	29,3	+ 15 46 9	9 32,9	28 10 55,1
30.	Lo. L. 28 42 5,9	29 22	63½	30,2	— 15 46,9	15 58,	28 10 51
July 4.	Up. L. 28 28 16,6	29,67	69	29,6	+ 15 46,9	33 40,1	28 10 53

Mean . . 28 10 52,5
Sun's parallax — 4,1

28 10 48,4
Nutation + 9,4

28 10 57,8
Error of the line of collimation . . + 6,3

Mean folftitial zenith diftance, 1746, 28 11 4,1

1748.

	Observed zenith distance	Barometer.	Thermometer.	Refraction.	Sun's semi-diameter.	Dist. from solstice.	Observations reduced to solstice.
1748.	° ′ ″			″ +	′ ″	° ′ ″	° ′ ″
June 15.	Up. L. 28 1 21,1	29,51½	64	29,3	+ 15 47,4	0 6 38,6	28 10 59,2
16.	Lo. L. 28 30 41,3	29,55	60	30,3	− 15 47,4	4 31,9	28 10 52,3
20.	Up. L. 27 54 45,7	29,86½	63½	29,5	+ 15 47,2	0 5,7	28 10 56,7
21.	Lo. L. 28 20 11,3	29,65½	75 +	29,2	− 15 47,1	0 1,3	28 10 52,1
22.	Up. L. 27 55 3,5	29,67	81½	28,2	+ 15 47	0 21,4	28 10 57,3
23.	Lo. L. 28 27 18,7	29,60½	72 −	29,3	− 15 47	1 6,7	28 10 54,3
24.	Up. L. 27 56 59,7	29,554	65	29	+ 15 46,9	2 16,7	28 10 58,9
29.	Lo. L. 28 40 29,3	29,90	64	30,5	− 13 46,9	14 18,7	28 10 55,2
				Mean			28 10 55,8
				Sun's parallax			− 4,1
							28 10 51,7
				Nutation			+ 6,1
							28 10 57,8
				Error of the line of collimation			+ 6,3
				Mean solstitial zenith distance, 1748,			28 11 4,1

	Observed zenith distance	Barometer.	Thermometer.	Refraction.	Sun's semi-diameter.	Dist. from solstice.	Observations reduced to solstice.
1766.	° ′ ″			″	′ ″	° ′ ″	° ′ ″
June 11.	Lo. L. 28 47 12	29,51	62 −	30,4	− 15 47,7	0 20 42,3	28 11 12,4
12.	Up. L. 28 11 38,3	29,38	61 −	29,6	+ 15 47,0	16 47,1	28 11 8,4
17.	Lo. L. 28 29 47,9	29,15½	66	29,1	− 15 47,3	3 20,2	28 11 9,3
21.	Up. L. 27 54 57,4	29,72	60¾	29	+ 15 47,1	0 29	28 11 13,5
22.	Lo. L. 28 26 39,6	29,71½	70¾	29,7	− 15 47	0 11,9	28 11 10,4
23.	Lo. L. 28 27 19,2	29,75	[illegible]	29,5	− 15 47	0 48,0	28 11 13,1
24.	Up. L. 27 50 45,7	29,80	[illegible]	28,4	+ 15 46,[illegible]	1 50,1	28 11 10,[illegible]
25.	Lo. L. 28 [illegible] 41,2	29,19	74	29,2	− 15 46,[illegible]	3 10,2	28 11 7,3
27.	Up. L. 28 2 18,5	29,40	67	28,9	+ 15 46,9	7 21,6	28 11 11,7
29.	Lo. L. 28 29 50,6	29,28	66	29,7	− 15 46,9	13 7	28 11 6,4
				Mean			28 11 10,5
				Sun's parallax			− 4,1
							28 11 6,4
				Nutation			+ 7,6
							28 11 14
				Error of the line of collimation			+ 2,6
				Mean solstitial zenith distance, 1766,			28 11 16,6

1743.

	Observed zenith dist. of Arcturus.	Refraction.	Aberration.	Nutation.	Precession.	Observations reduced.
1743.	° ′ ″	″	″	″	″	° ′ ″
May 12.	31 6 57,8	34	−1,9	+1,4	+2,1	31 7 33,4
17.	31 6 57,8	34	−0,8	+1,4	+1,8	31 7 34,2
18.	31 6 57,8	34	−0,6	+1,4	+1,8	31 7 34,4
June 5.	31 6 52,3	34	+2,9	+1,2	+0,9	31 7 31,3
9.	31 6 50,3	34	+3,8	+1,2	+0,7	31 7 30
10.	31 6 52	34	+4	+1,2	+0,6	31 7 31,8
16.	31 6 57	34	+5,3	+1,2	+0,3	31 7 37,8
18.	31 6 54,8	34	+5,5	+1,2	+0,1	31 7 35,6
25.	31 6 52	34	+6,7	+1,2	−0,2	31 7 33,7
July 1.	31 6 52	34	+7,8	+1,2	−0,5	31 7 34,5

Mean	31 7 33,6
Error of the line of collimation	+ 2
Mean zenith distance of Arcturus, June 21, 1743, . .	31 7 35,6
Mean zenith distance of the Sun's center, June 21, 1743,	28 11 2,8
Mean distance of Arcturus from the Sun's center, 1743,	2 56 32,8

1746.	° ′ ″	″	″	″	″	° ′ ″
June 4.	31 7 51,1	34	+2,5	−5,8	+1,1	31 8 22,9
21.	31 7 53,1	34	+6,1	−5,9	0	31 8 27,3
22.	31 7 50,2	34	+6,3	−5,9	0	31 8 24,6
23.	31 7 52,3	34	+6,4	−5,9	−0,1	31 8 27,7
Oct. 9.	31 7 58,6	34	+8,9	−6,4	−5,7	31 8 29,4

Mean	31 8 26,4
Error of the line of collimation	+ 6,3
Mean zenith distance of Arcturus, June 21, 1746, . .	31 8 32,7
Mean zenith distance of the Sun's center, June 21, 1746,	28 11 4,1
Mean distance of Arcturus from the Sun's center, 1746,	2 57 28,6

	Observed zenith dist. of Arcturus.	Barometer.	Thermometer.	Refraction.	Aberration.	Nutation.	Precession.	Observations reduced.
1766.	° ′ ″			″	″	″	″	° ′ ″
May 13.	31 14 20,7	29,46	43	34,9	−1,9	−7,6	+2	31 14 48,1
21.	31 14 23,1	29,81	46	35,1	−0,2	−7,6	+1,7	31 14 52,1
22.	31 14 19,9	29,67	49	34,7	0	−7,6	+1,6	31 14 48,6
June 23.	31 14 20,8	29,90	70	33,1	+6,4	−7,7	−0,1	31 14 52,5
	Mean							31 14 50,3
	Error of the line of collimation							+ 2,6
	Mean zenith distance of Arcturus, June 21, 1766, . .							31 14 52,9
	Mean zenith distance of the Sun's center, June 21, 1766,							28 11 16,6
	Mean distance of Arcturus from the Sun's center, 1766,							3 3 36,3

From the foregoing obfervations, it appears that the mean folftitial zenith diftance in fummer was as follows.

				Variation in 100 years.
			″	″
1743.	28	11	2,8	60
1746.	28	11	4,1	62,5
1748.	28	11	4,1	69,4
1766.	28	11	16,6	

And, by comparing the three former with the latter, the variation of the obliquity of the ecliptic in 100 years is as is expreffed in the laft column of the table.

By comparing the diftance of Arcturus from the center of the Sun in 1743, with the fame diftance as obferved in 1766 (an allowance being made for the proper motion of the ftar during the interval, as alfo for its variation in declination arifing from the pre-
ceffion

ceſſion of the equinoxes), it appears that its diſtance is 17″,3 leſs than it would have been, if the diſtance of the Sun's center from the equator had remained unvaried. By that quantity, therefore, the obliquity of the ecliptic has altered in 23 years; which is at the rate of 75″,2 in 100 years.

By comparing, in like manner, the diſtance in 1746, the obliquity of the ecliptic has diminiſhed 15″,6 in 20 years, or 78″ in 100 years.

Diſtance in 1743	2	56	32,8	In 1746.	2	57	28,6
Motion of the ſtar in decl. Southward		7	20,8				
Computed diſtance in 1766 . .	3	3	53,6		3	3	51,9
Obſerved diſtance in 1766 . . .	3	3	36,3		3	3	36,3
Variation of obliquity			17,3				15,6

The foregoing deductions prove, I think, beyond all doubt, that the obliquity has become leſs; but as the interval of time between the two terms of compariſon is ſo ſhort, that the errors committed in obſerving may bear a ſenſible proportion to the ſmall quantities juſt now found, and which, perhaps, are ſomewhat too large; let us have recourſe to Mr. Flamſteed's obſervations, and compare them with obſervations made by myſelf, in the courſe of the laſt and preſent years. For this purpoſe, I have reduced all the obſervations of the Sun, made in 1690, from May 26 to June 24, O. S. and alſo all the obſervations of Arcturus, made in the ſame year, to their mean poſition at the ſummer ſolſtice of that year. The obſervations, together with my own made at Oxford, are as follows.

	Observed zen. dist of the Sun's limbs.	Refraction.	Sun's semi-diameter.	Dist. from solstice.	Observations reduced.
1690.	° ′ ″	″	′ ″	° ′ ″	° ′ ″
May 26.	Lo. L. 29 6 20	31,8	15 46,4	0 50 26,1	28 0 39,3
	Up. L. 28 34 50	31,1	15 46,4		41,4
June 2.	Lo. L. 28 31 35	31,1	15 45,7	0 15 28,3	52,1
	Up. L. 28 0 5	30,4	15 45,7		52,8
3.	Lo. L. 28 28 20	31	15 45,6	0 12 8,6	56,8
	Up. L. 27 56 45	30,3	15 45,6		51,1
4.	Lo. L. 28 25 15	30,9	15 45,5	0 9 10,9	49,5
	Up. L. 27 53 55	30,3			28 0 59,9
6.	Lo. L. 28 20 40	30,8	15 45,5	0 4 30,1	28 0 55,2
	Up. L. 27 49 15	30,2			28 1 0,6
7.	Up. L. 27 47 10	30,1	15 45,4	0 2 46,8	28 0 38,7
	Lo. L. 28 18 40	30,8			28 0 38,6
10.	Lo. L. 28 16 10	30,7	15 45,3	0 0 5,5	28 0 49,9
	Up. L. 27 44 40	30,1			28 0 49,9
12.	Up. L. 27 44 50	30,1	15 45,3	0 0 22	28 0 43,4
	Lo. L. 28 16 45	30,8			28 1 8,5
13.	Lo. L. 28 17 17,5	30,8	15 45,1	0 1 7,4	28 0 55,8
	Up. L. 27 45 50	30,1			28 0 58,8
14.	Up. L. 28 13 30	30,8	15 45,1	0 2 17,6	28 0 58,1
	Lo. L. 27 46 50	30,1			28 0 47,6
16.	Up. L. 27 50 40	30,2	15 45,1	0 3 51,8	28 1 3,5
	Lo. L. 28 22 15	30,9			28 1 9
17.	Up. L. 27 53 5	30,2	15 45	0 8 16,4	28 1 3,8
	Lo. L. 28 24 30	30,9			28 0 59,5
20.	Lo. L. 28 34 15	31,1	15 45	0 17 56,4	28 1 4,7
	Up. L. 28 2 45	30,4			28 1 4
24.	Up. L. 28 21 5	30,8	15 45,1	0 36 30,6	28 0 50,3

Mean	28 0 54,2
Error of the line of collimation	1 30
	27 59 24,2
Sun's parallax	− 4,1
	27 59 20,1
Nutation	+ 9,5
Mean solstitial zen. dist. of the Sun's center, June 11, 1690, O. S.	27 59 29,6

1690.

	Observed zenith dist. of Arcturus.	Refraction.	Aberration.	Nutation.	Precession.	Observations reduced.
1690.	° ′ ″	″	″	″	″	° ′ ″
Feb. 14.	30 39 20	33,8	—12,2	—5,1	— 2,3	30 39 34,2
Apr. 13.	30 39 20	33,8	— 5,5	—5,3	— 5,4	30 39 37,6
25.	30 39 10		— 3,3	—5,4	— 6	29,1
26.	30 39 15		— 2,9	—5,4	— 6,1	34,4
May 13.	10		+ 0,4	—5,5	— 6,9	31,8
14.	10		+ 0,6	—5,5	— 7	31,9
15.	30 39 10	33,8	+ 0,8	—5,5	— 7	32,1
20.	10		+ 1,9	—5,5	— 7,3	32,9
22.	10		+ 2,3	—5,5	— 7,4	33,2
24.	30 39 5		+ 2,7	—5,6	— 7,5	28,4
June 12.	10	33,8	+ 6,4	—5,6	— 8,5	36,1
13.	10		+ 6,6	—5,6	— 8,6	36,2
16.	30 39 10		+ 7,1	—5,6	— 8,7	36,6
17.	10		+ 7,2	—5,6	— 8,8	30 39 33,4
July 1.	30 39 10	33,8	+ 9,4	—5,7	— 9,5	30 39 30,6
Dec. 13.	30 39 35	33,8	— 6,6	—6,5	—18,1	30 39 37,6
14.	30 39 40	33,8	— 6,8	—6,5	—18,2	30 39 42,3

Mean, January 1, 1690, O. S.	30 39 34
Preceffion to June 11, 1690	+ 8,4
Mean zenith diftance of Arcturus, June 11, 1690 . .	30 39 42,4
Mean folftitial zenith diftance of the Sun's center, June 11, 1690	27 59 29,6
Mean diftance of Arcturus in declination from the Sun's center, 1690	2 40 12,8

	Obferved zenith diftance of the Sun's upper limb.	Barometer.	Thermometer.	Refraction.	Sun's femi-diameter.	Dift. from folftice.	Obfervations reduced.
	° ′ ″			″	′ ″	° ′ ″	° ′ ″
1771. June 8.	28 36 47,6	30,08½	64⅓	30,9	15 48	0 36 0,4	28 17 6,1
14.	28 11 38,1	29,94	63 —	30	15 47,4	10 46,6	8,9
18.	28 3 4,4	29,75	61 +	29,7	15 47,2	2 8,3	13
21.	28 0 55,4	30,08⅔	65⅓	29,7	15 47	0 0,8	11,3
22.	28 1 1,4	30,07	66 +	29,6	15 47	0 7,6	28 17 10,4
24.	28 2 26,1	30,07	62	30	15 46,9	1 35,8	7,2
25.	28 3 46,9	30,05	69	29,5	15 46,9	2 [illegible]	6,5
26.	28 5 38,1	29 97½	68	29,5	15 46,9	[illegible]	11,8
29.	28 13 21,3	29,84	61 —	30,2	15 46,9	[illegible]7,7	28 17 10,7

Mean	28 17 8,7
Sun's parallax	4,1
	28 17 4,6
Nutation	— 6,8
	28 16 57,8
Error of the line of collimation	+ 4,8
Mean folftitial zenith diftance of the Sun's center, 1771,	28 17 2,6

1772.

		Observed zenith distance of the Sun's limbs.	Barometer.	Thermometer.	Refraction.	Sun's semi-diameter.	Dist. from solstice.	Observations reduced.
		° ′ ″			″	′ ″	° ′ ″	° ′ ″
1772. June 8.	Lo. L.	29 4 31,3	30,18	71	30,7	−15 48	0 31 59,9	28 17 14,1
9.	Up. L.	28 28 1	30,05½	67½	30,1	+15 47,9	27 5,4	28 17 13,6
11.	Lo. L.	28 51 5,5	30,10½	71¼	30,3	−15 47,7	18 29,2	18,9
12.	Lo. L.	28 47 15,7	30,22⅔	60⅔	31,2	−15 47,6	14 48,4	10,9
14.	Up. L.	28 9 34,1	30,15½	63	30,1	+15 47,4	8 38,4	13,5
	Lo. L.	28 41 11,1			30,8			16,1
15.	Up. L.	28 7 5,4	29,91½	72	29,2	+15 47,3	6 10,7	11,2
	Lo. L.	28 38 40,8			29,8			28 17 12,6
16.	Up. L.	28 5 0,3	29,82½	70¾	29,1	+15 47,3	4 7,6	9,1
	Lo. L.	28 36 38,6			29,8			13,5
18.	Up. L.	28 2 16,4	30,02¾	71½	29,2	+15 47,2	1 15,6	17,2
	Lo. L.	28 33 48,1			29,7			15
19.	Up. L.	28 1 26,3	29,97	75⅓	28,8	+15 47,1	0 26,8	15,4
	Lo. L.	28 32 55,8			29,5			11,4
20.	Lo. L.	28 32 30,3	29,93	79	29,1	−15 47,1	0 2,8	28 17 9,5
21.	Lo. L.	28 32 32,8	29,79	72	29,6	−15 47	0 3,5	11,9
22.	Up. L.	28 1 24,8	30,00¼	73	29	+15 47	0 29,9	28 17 10,9
23.	Up. L.	28 2 14,4	30,01	76¼	28,8	+15 47	1 19,4	28 17 10,8
	Lo. L.	28 33 54,1			29,5			17,2
26.	Up. L.	28 7 20,3	29,96	85 —	28,2	+15 46,9	6 18,9	16,5
	Lo. L.	28 38 50,4			28,8			28 17 13,4

Mean	28 17 13,4
Sun's parallax	— 4,1
	28 17 9,3
Nutation	— 8,7
	28 17 0,6
Error of the line of collimation	+ 4,8
Mean solstitial zenith distance of the Sun's center, 1772,	28 17 5,4

	Observed zenith distance of Arcturus.	Barometer.	Thermometer.	Refraction.	Aberration.	Nutation.	Precession.	Observations reduced.
	° ′ ″			″	″	″	″	° ′ ″
1772.								
July 11.	31 21 53,6	29,87	73	32,9	+ 9,4	+ 1,9	— 10,1	31 22 27,7
Aug. 11.	31 21 56,6	29,91	64 —	33,8	+ 12,2	+ 2,1	— 11,7	33
17.	50,6	29,93	75 +	32,8	+ 12,2	+ 2,1	— 12	25,7
19.	54,6	29,76	77 —	32,5	+ 12,3	+ 2,2	— 12,1	29,5
Oct. 16.	31 21 58,1	29,99½	61	34,2	+ 7,7	+ 2,6	— 15,1	27,2
Nov. 2.	31 22 3,3	29,32¼	49 —	34,5	+ 4,4	+ 2,7	— 16	28,9
7.	22 3,5	29,25	49	34,3	+ 3,8	+ 2,7	— 16,3	28
12.	8,3	29,82	38	35,9	+ 2,1	+ 2,7	— 16,5	32,5
20.	31 22 12,3	29,85	46 +	34,1	+ 0,4	+ 2,7	— 16,9	31 22 32,6
21.	10,3	29,52 +	42 —	35,4	+ 0,2	+ 2,7	— 17	31,6
23.	11,3	29,37	40	35,2	— 0,2	+ 2,7	— 17,2	31,8
Dec. 10.	15,3	29,62	39⅓	35,7	— 3,9	+ 2,9	— 18	32
16.	31 22 12,3	29,69	45¼	35,1	— 5	+ 2,9	— 18,3	31 22 27

	° ′ ″
Mean zenith distance of Arcturus, January 1, 1772	31 22 29,8
Precession to June 21, 1772	+ 9
Mean zenith distance of Arcturus, June 21, 1772	31 22 38,8
Error of the line of collimation	+ 4,2
True mean zenith distance of Arcturus, June 21, 1772	31 22 43
Mean zenith distance of the Sun's center, June 21, 1772	28 17 5,4
Mean distance of Arcturus in declination from the Sun's center, June 21, 1772	3 5 37,6

	° ′ ″
Mean distance in June 1690 . .	2 40 12,8
Precession, &c. to June 1772 . .	26 16,4
Computed distance in June 1772 .	3 6 29,2
Observed distance in June 1772 .	3 5 37,6
Diminution of obliquity in 82 years	51,6

From

From the foregoing obſervations it appears that, at the ſummer ſolſtice of the year 1690, Arcturus was 2° 40′ 12″,8 to the South of the Sun's center in declination: the motion of the ſtar, in declination, from that time to the ſummer ſolſtice of the year 1772, including its proper motion, is 26′ 16″,4. Arcturus, therefore, in 1772, ſhould have been 3° 6′ 29″,2 to the South of the Sun's center, if the angle of the ecliptic and equator had not varied: but that diſtance was found by actual obſervation to be 51″,6 leſs. By ſo much therefore muſt the obliquity of the ecliptic have become leſs in an interval of 82 years; and, conſequently, the variation in 100 years will be 62″,92.

If the obſervations of Arcturus be reduced to the ſolſtice of 1771, and the zenith diſtance of the Sun's center, as obſerved in that year, be made uſe of in the ſame manner, the variation of the obliquity in 81 years will be found = 48″,8, and in 100 years = 60″.

If the quantity of the arc of Mr. Flamſteed's inſtrument were accurately known, the obſervations which he made at the winter ſolſtice in 1690 might be compared with later obſervations, in order to determine both the quantity of the obliquity in 1690, and alſo the variation ſince his time. Accordingly, I have endeavoured to determine the error of the arc of the inſtrument between 28° and 75° of zenith diſtance, and proceeded in the following manner. I computed ſeveral obſervations of the ſtars ζ Tauri, η Pleïadum, η and μ Geminorum, and ϕ, σ, and o Sagittarii, as obſerved by Mr. Flamſteed, in the years 1690, 1691, and 1692, and reducing them

 to

to the years 1760 and 1766, I compared the differences of declination between thoſe ſtars, reſulting from Mr. Flamſteed's obſervations, with the differences given by the places of the ſame ſtars, as ſettled by Dr. Bradley in 1760, and alſo by actual obſervations of the ſame ſtars made at Shirburn-Caſtle in 1766; and, by combining theſe differences together, I found that the whole arc of 90 degrees was too ſhort by 43″. Suppoſing the error to be uniform, the proportional part of this quantity, thus found for the ſolſtitial zenith diſtance of the Sun in June = 13″,4, is nearly confirmed upon the authority of Mr. Flamſteed himſelf, who, in the prolegomena to the third volume of the Hiſtoria Cœleſtis, where he is deducing the latitude of the Royal Obſervatory at Greenwich, and the quantity of the obliquity in 1690, from his own obſervations, allows the zenith diſtances at 28°, 36°, and 40°, on his inſtrument, to be too ſmall by 15′ and by 20″, at 75°.

I have therefore computed the obſervations of the Sun, made from November 30 to December 20 of 1690, which, reduced to the ſolſtice, are as in the following table; to which are ſubjoined the obſervations made by myſelf at Oxford, at the winter ſolſtice of 1771.

1690.

	Observed zenith distance of the Sun's limbs.	Refraction. +	Sun's semidiameter.	Dist. from solstice. +	Observations reduced.
1690.	° ′ ″	′ ″	′ ″	° ′ ″	° ′ ″
Nov. 30.	Lo. L. 74 45 17,5	3 27,9	— 16 16,6	0 25 43,1	74 58 11,9
	Up. L. 74 12 40	3 19,6	+ 16 16,6		74 57 59,3
Dec. 2.	Lo. L. 74 54 5	3 28,7	— 16 16,6	17 16,1	74 58 33,2
2.	Up. L. 74 21 25	3 21,7			19,4
10.	Lo. L. 75 11 0	3 32,7	— 16 17,2	0 4,6	20,1
	Up. L. 74 38 35	3 25,4			22,2
13.	Lo. L. 75 9 50	3 32,4	— 16 17,2	1 24,4	74 58 29,6
	Up. L. 74 37 15	3 25,3			21,9
15.	Lo. L. 75 6 45	3 31,6	— 16 17,3	4 39,4	74 58 38,7
	Up. L. 74 34 5	3 24,5			26,2
17.	Lo. L. 75 1 30	3 30,2	— 16 17,3	9 46,9	29,8
20.	Up. L. 74 13 15	3 19,8	+ 16 17,3	25 38,8	74 58 30,9
	Lo. L. 74 46 5	3 27	— 16 17,3		74 58 53,5

Mean	74 58 25,9
Error of the line of collimation	— 1 10
	74 57 15,9
Sun's parallax	— 8,5
	74 57 7,4
Nutation	— 9,6
Mean solstitial zenith distance of the Sun's center, Dec. 1690,	74 56 57,8

 1771.

	Observed zenith distance of ☉'s upp. limb.	Barometer.	Thermometer.	Refraction.	Sun's semidiameter. +	Dist. from solstice. +	Observations reduced.
	° ′ ″			′ ″	′ ″	° ′ ″	° ′ ″
1771. Dec. 8.	74 11 1,7	29,43	44¼	3 21	16 18	0 42 26,3	75 13 12,2
12.	74 32 25,4	29,56⅓	47 +	3 25,2	16 18,5	21 3,4	17,7
16.	74 46 32,3	28,68	45⅔	3 22,7	16 18,7	7 2,5	21,8
19.	74 51 57,3	29,23	42	3 29,6	16 19	1 26,1	17,2
24.	74 51 48,5	29,07	40	3 29,6	16 19,1	1 31	13,4
25.	74 50 24,2	29,43	41¼	3 31,2	16 19,2	2 56,9	16,7
27.	74 46 13,3	29,42½	48½	3 26,6	16 19,2	7 13,4	17,7
30.	74 36 13,9	30,13	39¼	3 34,1	16 19,3	17 9,3	75 13 21,8

Mean	75 13 17,3
Sun's parallax	− 8,5
	75 13 8,8
Nutation	+ 7,9
	75 13 16,7
Error of the line of collimation	+ 4,8
Mean solstitial zenith distance of the Sun's center, December 1771,	75 13 21,5

The mean obliquity of the ecliptic resulting from the zenith distances, as observed at the two solstices in 1690, by applying the known latitude of the place, will be found to be widely different, if no correction be applied for the error of the instrument.

June, zenith distance	−27 59 29,6	Dec. zen. dist.	74 56 57,8
Latitude of Greenwich	51 28 38		−51 28 38
	23 29 8,4		23 28 19,8

But if the observations be corrected by the error of the instrument, the two results will be found to agree together as nearly as can be expected.

			″				″
	27	59	29,6		74	56	57,8
		+	13,4			+	35,8
	27	59	43		74	57	33,6
	51	28	38		—51	28	38
Obliquity	23	28	55		23	28	55,6

Or, if the obliquity be required independent of a knowledge of the latitude of the place, it will be found to be = 23° 28′ 55″,3.

	°	′	″
December	74	57	33,6
June	—27	59	43
Difference	46	57	50,6
Mean obliquity 1690, ½ Diff.	23	28	55,3

By commparing the obfervations at the fummer folftices of 1771 and 1772 with thofe at the winter folftice of 1771, it appears that the mean obliquity was about the beginning of the year 1772 = 23° 28′ 9″,4 and 23° 28′ 8″. I fuppofe therefore the mean obliquity to be 23° 28′ 8″ at the beginning of the prefent year; and confequently, the obliquity has diminifhed, by my obfervations, 47″ in 81 years, fince Mr. Flamfteed's time, or at the rate of 58″ in 100 years, a quantity which will be found nearly at a mean of the computations framed by Mr. Euler and Mr. de la Lande, upon the principles of attraction.

Oxford, Dec. 23, 1772.

XV. *New*

XV. *New Obſervations upon Vegetation. By Mr.* Muſtel *of the Acad. of Sciences at* Rouen; *tranſlated from the* French.

Read Jan. 14, 1773. MANY celebrated writers, induced by the analogy, which they obſerved betwixt the vegetable and animal kingdoms, have admitted the circulation of the ſap in the one, in a ſimilar manner to the circulation of the blood in the other.

This important point of vegetable œconomy produced a diverſity of opinions, and has not yet been ſufficiently cleared up.

Dr. Hales, in his Vegetable Staticks, does not ſeem to embrace the ſyſtem of the circulation of the ſap; but he does not prove the contrary *.

* *Il ne prouve pas contre.* This certainly is a miſtake. Dr. Hales, in the IVth Chapter of his *Phyſical Staticks*, not only declares openly againſt the doctrine of the circulation of the ſap, and overturns the arguments alledged in favour of this opinion; but he produces ſeveral new experiments, which prove directly the impoſſibility of ſuch a circulation. (See p. 144, &c.) His reaſons have been thought ſo convincing, that the ſyſtem of the circulation in plants has been ever ſince exploded in England; and that they have had a ſimilar effect abroad, appears from the following quotation from a book of the ingenious Mr. Bonnet, F. R. S. of Geneva, intitled *Recherches ſur l'uſage des feuilles*, printed in 1754, p. 269. " Pour moi, perſuadé de la fauſſeté de " cette opinion (que la ſeve circuloit comme le ſang) par les ex- " périences de M. Hales (Ch. IV.) &c." M. M.

Mr.

Mr. Du-Hamel, in his Phyſiology of Trees, contents himſelf with relating what has been ſaid for or againſt this opinion; but, though he ſufficiently hints that he does not believe it true, he determines nothing about it. The friends of the circulation in plants have never been able to find in them any thing analogous to that powerful organ, which is the promoter of it in animals; for want of ſuch an organ, they were forced to imagine valves and paps in the lymphatick veſſels of plants, by means of which the liquors once introduced into the ſap veſſels were ſuppoſed to be hindered from going back; but, unfortunately, no body has ever been able to diſcover theſe valves and paps, ſo different from the ſimple contrivances, by which nature is uſed to arrive at her ends.

An experiment, which I made, and of which I propoſe giving an account in this paper, throws a great light upon this queſtion, as well as upon ſeveral others; and the concluſions deducible from it appear to me deciſive.

On the 12th of January, I placed ſeveral ſhrubs in pots againſt the windows of my hot-houſe, ſome within the houſe, and others without it. Through holes made for this purpoſe in the panes of glaſs, I paſſed a branch of each of the ſhrubs, ſo that thoſe on the inſide had a branch without, and thoſe on the outſide one within; after this, I took care that the holes ſhould be exactly cloſed and luted. This inverſe experiment, I thought, if followed cloſely, could not fail affording ſufficient points of compariſon, to trace out the differences, by the obſervation of the effects.

The

The 20th of January, a week after this diſpoſition, all the branches that were in the hot-houſe began to diſcloſe their buds. In the beginning of February, there appeared leaves, and towards the end of it, ſhoots of a conſiderable length, which preſented the young flowers. A dwarf apple-tree and ſeveral roſe-trees, being ſubmitted to the ſame experiment, ſhewed the ſame appearance then as they commonly put on in May; in ſhort, all the branches which were within the hot-houſe, and conſequently kept in the warm air, were green at the end of February, and had their ſhoots in great forwardneſs. Very different were thoſe parts of the ſame tree, which were without and expoſed to the cold. None of theſe gave the leaſt ſign of vegetation; and the froſt, which was intenſe at that time, broke a roſe pot placed on the outſide, and killed ſome of the branches of that very tree, which, on the inſide, was every day putting forth more and more ſhoots, leaves, and buds, ſo that it was in full vegetation on one ſide, whilſt frozen on the other.

The continuance of the froſt occaſioned no change in any of the internal branches. They all continued in a very briſk and verdant ſtate, as if they did not belong to the tree, which, on the outſide, appeared in the ſtate of the greateſt ſuffering. On the 15th of March, notwithſtanding the ſeverity of the ſeaſon, all was in full bloom. The apple-tree had its root, its ſtem and part of its branches, in the hot-houſe. Theſe branches were covered with leaves and flowers; but the branches of the ſame tree, which were carried to the outſide, and expoſed to the cold air, did not in the leaſt partake

partake of the activity of the reſt, but were abſolutely in the ſame ſtate, which all trees are in during winter. A roſe tree, in the ſame poſition, ſhewed long ſhoots with leaves and buds; it had even ſhot a vigorous branch upon its ſtalk, whilſt a branch which paſſed through, to the outſide, had not begun to produce any thing, but was in the ſame ſtate with other roſe-trees left in the ground. This branch is four lines in diameter, and eighteen inches high.

The roſe-tree on the outſide was in the ſame ſtate; but one of its branches drawn through to the inſide of the hot-houſe, was covered with leaves and roſe-buds. It was not without aſtoniſhment that I ſaw this branch ſhoot as briſkly as the roſe-tree which was in the hot-houſe, whoſe roots and ſtalk, expoſed as they were to the warm air, ought, it ſhould ſeem, to have made it get forwarder than a branch belonging to a tree, whoſe roots, trunk, and all its other branches were at the very time froſt-nipt. Notwithſtanding this, the branch did not ſeem affected by the ſtate of its trunk; but the action of the heat upon it produced the ſame effect as if the whole tree had been in the hot-houſe.

It would be uſeleſs to give an account of the diary I kept throughout the courſe of this intereſting experiment. It may be ſufficient to obſerve, that the walk of nature was uniformly the ſame. The interior branches continued their productions in a regular manner, and the external ones began theirs at the ſame time, and in the ſame manner, as they would have done, had they been left in the ground. The fruits of the interior

terior branches of the apple-tree were, in the beginning of May, of the ſize of nutmegs; whilſt the bloſſoms but juſt began to ſhew themſelves on the branches without. I ſhewed Mr. Du-Tillet, of the Academy of Sciences at Paris, on his paſſage through this town, the effects of my experiments, and likewiſe communicated to him another obſervation, which chance occaſioned, and ought not to be omitted.

I obſerved that three of the flower buds of the apple-tree had been gnawed off by a ſnail in ſuch a manner, that all the petals and ſtamens had diſappeared, being eat up cloſe to the calyx. This not having been entered by the ſnail, the baſis of the piſtillum and the embryo were preſerved.

I took it for granted that theſe flowers would bear nothing; but I was ſoon convinced of my miſtake. Almoſt all of them bore fruit; the apples were perfectly formed, and ſix or ſeven pretty large ones too were ſeen upon each bunch. On the other hand, the ſnail had ſpared ſome other bunches, (doubtleſs becauſe more difficult to be got at;) but out of ten or twelve flowers in each bunch, not above one or two ſhewed any ſigns of fruit. This ſuggeſted to me the idea, that, when the flowers of trees are full blown, the prevention of the natural fall of the petals and ſtamens gives a greater aſſurance of the fructification; and on ſeveral times repeating the following experiment, I convinced myſelf that it did ſo. In imitation of the ſnail, I cut with my ſciſſars the petals of apple, pear, plumb, and cherry bloſſoms, cloſe to the calyx. Almoſt every one of thoſe, which

which were thus cut, ſucceeded, whilſt ſeveral of the neighbouring flowers miſ-carried.

Thus did a ſnail teach me how to render a tree fruitful; nor is it the firſt time that animals have been the inſtructors of mankind. I confeſs, however, that this proceſs is not very practicable in a large orchard: but it might be adopted in an eſpalier; in which one would chuſe to procure a great deal of fruit from trees of the beſt ſort. It may indeed be queſtioned, whether the ſuppreſſion of the ſtamens would not render the fruit barren; and in fact I found, that, though the flowers of the dwarf apple-tree, whoſe petals and ſtamens were eat up by the ſnail, gave me apples equally large and beautiful, and that, when I came to open them, I found the capſules formed as uſual at the center of them; yet they were entirely empty, without the leaſt appearance of a pip. Abſolute fructification conſequently did not take place; ſince botaniſts, with reaſon, call nothing fruit but the ſeed, which contains the germen, which is to perpetuate the ſpecies. All the other parts, being only intended to co-operate in the formation and preſervation of the ſeeds, periſh of courſe, when once the ſeeds are come to maturity and perfection, and the work of nature fulfilled.

Another remarkable thing in theſe apples is, that in the upper part there was found a much deeper cavity than uſual. It was eight or nine lines deep. The orifice of this cavity was bordered by five tubercles, indented and ſomewhat elevated; but there was no veſtige of the calyx, which, it is well known, remains always to the

 upper

upper part of apples and pears, and is commonly called the eye.

I now return to my firſt experiment; the conſequences of which, as I have deſcribed them, ſeem to prove,

I. Firſt that the circulation of the ſap does not take place in plants, as the circulation of the blood in animals. This may be deduced from the following obſervations.

The tree in the hot-houſe went through all its changes during the winter, and the branch expoſed to the open air underwent none; conſequently the ſap, which was in action in the root, ſtock, and head, of the tree, did not circulate through the branch without; which had no ſhare in the vegetation of the roots and trunk. It might, indeed, be argued that the cold air, to which this branch was expoſed, ſtopped the circulation, and therefore that the firſt experiment would not be deciſive; but the inverſe of it ſeems fully ſo.

The tree placed on the outſide of the hot-houſe continued, during the whole winter, in the ſtate of numbneſs, natural to all trees, which are expoſed at that ſeaſon; but one of its branches, which was in the hot-houſe, put forth ſucceſſively its buds, leaves, bloſſoms, and fruits. Whilſt therefore the root of the tree, to which this branch belonged, was in the ground ſo frozen, that the pot itſelf, in which it ſtood, was broken by it, whilſt the ſtock and top of the tree were ſo covered over with ice, that many of the branches were killed; this branch alone did not in the leaſt partake of the common ſtate of numbneſs and ſuffering, and was on the contrary in full vegetation.

tion. The ſap in it muſt have been extremely rarefied, and in very quick motion, whilſt that of the tree was greatly condenſed, and in total inaction. How is it poſſible to conceive a circulation of the ſap from ſuch a frozen root and ſtock, to a branch full of vigour, and loaded with leaves and flowers? Surely this experiment muſt appear concluſive againſt the ſyſtem of circulation; ſince in this caſe it could at beſt only be admitted to have taken place in the vegetating branch; and that would very improperly be termed circulation, which ſhould be confined to one limb.

II. This experiment proves, that each part of a tree is furniſhed with a ſufficient quantity of ſap to effect the firſt production of buds, flowers, and fruits. There is little probability that the branch drawn into the hot-houſe ſhould have derived its ſap from the roots of the tree: as they, at that time, lay in a very ſmall quantity of earth, rendered extremely hard and dry by the froſt, they could have but little liquor to ſpare; and even this, conſidering the congealed ſtate of the lymphatick veſſels of the ſtock, could have found no paſſage to the branch. This branch muſt of courſe have been enabled to continue its vegetation by the quantity of ſap with which it was provided, the conſumption of which muſt have been ſupplied at the firſt breaking of the froſt. This truth, now demonſtrable by experience, had been pointed out before by a multiplicity of other facts. Every body may have obſerved that a tree, which has been blown down in autumn, though ſeparated from its trunk, begins the ſame vegetation, that it would have done if it had remained ſtanding.

ing. Its buds open, it bears leaves, and even ſhoots, which ſometimes are very long, and muſt be the effects of the ſap it contained. It is true, indeed, that this appearance does not continue long, becauſe the proviſion of ſap once exhauſted, without being renewed, every thing muſt of neceſſity periſh.

An effect of the like kind often deceives us in trees, that have been newly planted, and in ſcions, which produce flowers and even fruits, without ever having taken root. But in this caſe the ſymptoms, which would ſeem to promiſe life, are on the contrary the forerunners of death; becauſe the leaves, being from their nature the moſt powerful organs of tranſpiration and diſſipation, the graft is the more readily exhauſted, when there is no root to furniſh it with a freſh ſupply of nutritive juices.

III. This experiment proves that it is heat, which unfolds the leaves, and produces the other parts of fructification, in the branch expoſed to its action.

Autumn is the time, in which Nature employs itſelf as it were clandeſtinely, under the cover of the leaves, in forming the buds, which contain the rudiments of the leaves, bloſſoms, and fruits, that are to be produced in the courſe of the ſucceeding ſummer. Theſe buds prepare and work themſelves out, during the winter, under the rough coats, that are deſtined to preſerve them from the injuries of the weather. As ſoon as the warm weather in the ſpring begins to be felt, the buds open, and their coats, which then become uſeleſs, drop off, and give place to the productions, which they

they contained and preſerved. Immediately after this, the bloſſoms, flowers and fruits make their appearance. This is the uſual operation; but in the caſe before us, nature was, as it were, ſurprized by art: what ſhe ſhould not have done till ſpring, ſhe did in the winter, becauſe the heat of the hot-houſe produced that expanſion, which, according to the natural courſe, ought to have been effected by the rays of the ſun darting leſs obliquely than before upon the horizon. There is no doubt but it is to heat, either natural or artificial, that this expanſion is owing; and the experiment proves that it is only in that part of the tree, which is expoſed to the effect of heat, that the ſap, which in every other part remains torpid and inactive, is put into motion, and produces vegetation. From this, it appears that the vegetable œconomy is different from the animal, and that thoſe, who endeavoured to eſtabliſh the circulation in both, carried their analogy too far.

This fact, now eſtabliſhed, furniſhes a good reaſon why in the tapping of the maple and ſugar-birch-trees, ſo much liquor runs out on one ſide, and none at all on the other. It is well known that, if during the time of a froſt, or a ſummer's day, towards noon, you bore a hole on the ſide of the maple-tree expoſed to the ſouth, you will get a great quantity of liquor from it; and that if you bore the north-ſide at the ſame time, you will not get a drop. The cauſe of this evidently appears from what has been ſaid. One likewiſe ſees why trees expoſed to the ſouth loſe a great many of their branches, and ſometimes die altogether, in the courſe of a ſevere winter; whilſt

trees of the ſame ſort, but placed to the north, or in ſome other expoſition, will ſtand the hardeſt froſts. This is particularly remarkable in the ever-greens, whoſe reſinous and oily ſap being liquefied by the heat of the ſun, the tree cannot eſcape ſuffering a great deal, whenever it is ſurprized in that ſtate by the night froſts. Thoſe obſervers, who attend to this, and know how well pines, firs, and bays ſucceed, when planted on the back of mountains expoſed to the north, will take care not to place ſuch kind of trees in a ſouthern aſpect, in hopes of their ſucceeding better by it.

Many other conſequences might be drawn from theſe experiments; but the bounds, I have aſſigned to this paper, do not allow it. I propoſe examining them more at large in a treatiſe upon vegetation, which, I hope, the obſervations and experiments I have made, may render intereſting and uſeful.

XVI. *Actual Fire and Detonation produced by the Contact of* Tin-foil, *with the Salt composed of Copper and the Nitrous Acid. By* B. Higgins, *M. D. Communicated by* R. Brocklesby, *M. D. F. R. S. and of the* Royal College *of* Physicians *in* London.

Read Dec. 24, 1772, and Jan. 28, 1773.

SEVERAL pieces of thin sheet-copper, placed vertically, and at a small distance from each other, in the strong nitrous acid diluted with half its quantity, or more, of water, and suffered to remain in a close vessel, until the acid is saturated, afford a crystalline blueish green salt, which is to be separated from the undissolved copper and the superfluent green liquor, and kept in a well-corked bottle; because, on exposure to the air, it deliquesces.

This salt, taken moist, but not very wet, and beaten to the fineness of basket sea-salt, in a mortar, is to be strewed to the thickness of a shilling, on a piece of tin-foil, twelve inches in length and three in breadth.

Then the foil is to be instantly rolled up, so as to include the salt, as it lay, between the coils. The ends are to be shut by pinching them toge-

ther, and the whole is to be preſſed flat and cloſe.

All this being done as quickly as poſſible, the firſt phænomenon is—A part of the ſalt deliqueſces.

2d. This part, impregnated with tin, changed in colour, and of a thicker conſiſtence, begins to froth forth from the ends of the coil.

3d. A ſtrong frothing, accompanied with moderate warmth.

4th. The emiſſion of copious nitrous fumes.

5th. Heat intolerable to the fingers.

6th. Exploſion and fire, which burſt and fuſe the tin-foil in ſeveral places, if it be very thin.

After many conjectures and experiments, I diſcovered a property in the cupreous ſalt, from which; and the known affinities of the bodies concerned, theſe appearances, however new and ſingular, may be accounted for.

The cupreous ſalt properly dried and placed where it may receive a heat, not much greater than what the hand can bear, takes fire. The circumſtances which favour this ignition, and contribute to produce it in the ſmalleſt degree of heat, concur in the following convenient method of trying the experiment.

A piece of ſoft bibulous paper is to be dipped in the nitrous ſolution of copper, and dried before the fire two or three times alternately. Then it is to be approached towards the heat, as near as can be borne, by the hand which holds it without pain: there, if it has been ſufficiently dried, it will preſently catch fire, and burn to a brown calx.

The

The eaſy ignition of the ſalt in a ſlight heat being thus aſcertained, there is no room to doubt that the foregoing phænomena are produced in the following manner.

The acid of the liquor, which moiſtened the ſalt, quits the copper to unite with the tin, leaving the water to be imbibed by the contiguous ſalt of copper, which then diſſolves, and acts briſkly on the tin-foil.

It is well known that the action of the nitrous acid on tin is always accompanied with conſiderable heat and effervеſcence, and that the ſolution of metallic ſalts in watry liquors is haſtened by heat.

In our experiment, the warmth generated by the firſt action of the cupreous ſolution, promotes the deliqueſcence of the cryſtallized ſalt. The union of the acid with the tin is rapid, not only as being aſſiſted by heat, but on account of the great ſurface expoſed; whence the ſtrong frothing, and the extraordinary heat, by which the redundant moiſture is carried away, and the undecompoſed part of the cupreous ſalt, together with that lately formed with the tin, perfectly dried.

The heat generated upon both ſurfaces of a large expanſe of tin, is concentrated by cloſely coiling it into a ſmall compaſs, and being retained by the various ſurrounding laminæ of metal, it is neceſſarily accumulated to a quantity, which, if we may judge from the touch, is more than ſufficient to fire the dry cupreous ſalt.

The ſalt formed with tin, and the nitrous acid, burns and ſparkles in a red heat. Catching fire, therefore, from the ignited cupreous ſalt, it burns

with it, and affifts in the detonation, which is common to all nitrous compofitions in fimilar circumftances.

If the falt be very wet, there will not be much fire or explofion, becaufe the heat will be diffipated before the falt can be fufficiently dried in every part.

If the falt be not moift, it cannot commence the action which is neceffary; and there will be no fire, becaufe there can be no hafty folution of the tin to give the requifite heat.

If the tin and falt be not coiled up in due time, there will be very little heat and no fire; becaufe the diffipation of the heat from a broad expanfe, keeps pace with the generation of it; and as the moifture exhales quickly in this manner, there is none left to renew the action on the tin and confequent heat, when the proper time of coiling has elapfed.

A piece of tin-foil, larger than that I have defcribed, cannot eafily be managed; fmaller pieces give lefs fire in the direct proportion of their furfaces, and the quantity of falt which they can, at the fame inftant, reduce to the required ftate of drynefs.

The fudden diffipation of the moifture appears to me the moft curious of thefe phænomena. To render it the more obfervable, I made the following experiments.

I placed a piece of tin-foil, twelve inches long by two broad, loofely coiled, and ftanding vertically on the flatteft end, in half a table-fpoonful of the faturated folution of copper in the diluted nitrous acid, and found that fcarce five feconds elapfed

elapſed from the time, when a briſk effervеſcence, accompanied with weak nitrous fumes, aroſe, until the liquor became a conſiſtent maſs, and ſparks of fire iſſued from the coils of tin; which having attracted part of the ſolution above the common level, brought it into the condition in which it is readily dried, heated, and fired.

A like quantity of the ſame ſolution, kept in a ſtrong boiling heat, does not acquire ſuch conſiſtence in a ten-fold ſpace of time.

The haſty exhalation, therefore, is not cauſed by the heat alone; neither does it ſeem to require any great ſurface. What elſe it is owing to, I commit for a while to the examination of the curious.

Orchard-Street, Portman-
Square, Dec. 23, 1772.

B. Higgins.

Received Nov. 12, 1772.

XVII. *Extracts of some Letters, from Sir* William Johnson *Bart. to* Arthur Lee, *M. D. F. R. S. on the Customs, Manners, and Language of the* Northern Indians *of* America.

Read Jan. 28, 1773.

IN all enquiries of this sort, we should distinguish between the more remote tribes, and those Indians, who, from their having been next to our settlements for several years, and relying solely on oral tradition for the support of their antient usages, have lost great part of them, and have blended some with our customs, so as to render it extremely difficult, if not impossible, to trace their customs to their origin.

The Indians did certainly live under more order and government formerly, than at present. This may seem odd, but it is true; for, their intercourse being with the lower class of our traders, they learn little from us but our vices; and their long wars, together with the immoderate use of spirituous liquors, have so reduced them, as to render that order, which was first instituted among them, unnecessary and impracticable.

They

They do not at preſent uſe hieroglyphics; their figures being drawn, to the utmoſt of their ſkill, to repreſent the thing intended. For inſtance, when they go to war, they paint ſome trees with the figures of warriors, often the exact number of the party; and if they go by water, they delineate a canoe. When they gain a victory, they mark the handle of their tomahawks with human figures, to ſignify priſoners; and draw the bodies without heads, to expreſs the ſcalps they have taken. The figures which they affix to deeds have led ſome to imagine, that they had alphabetical characters or cyphers. The fact is this. Every nation is divided into tribes, of which ſome have three, as the turtle, bear, and wolf; to which ſome add the ſnake, deer, &c. Each tribe forms a little community within the nation; and as the nation has its peculiar ſymbol, ſo has each tribe the particular badge from which it is denominated: and a Sachem of each tribe being a neceſſary party to a fair conveyance, ſuch Sachem affixes the mark of his tribe thereto, like the public ſeal of a corporation. With reſpect to the deed of 1726, of which you ſent me the ſignatures, the tranſaction was in ſome meaſure of a partial nature. All the nations of the confederacy did not ſubſcribe it; and thoſe chiefs who did, neglected to pay due regard to their proper ſymbols; but ſigned agreeably to fancy, of which I have ſeen other inſtances. The manner I have mentioned is the moſt authentic, and conformable to their original practice.

As to the information, which, you obſerve, I formerly tranſmitted to the Governor of New York,

York, concerning the belt and 15 bloody ſticks ſent by the Miſſiſagees, the like is very common; and they uſe theſe ſticks, as well to expreſs the alliance of caſtles, as the number of individuals in a party. The ſticks are generally about 6 inches in length, very ſlender, and painted red if the ſubject be war. Their belts are moſtly black wampum, painted red when they denote war. They deſcribe caſtles ſometimes upon them, by ſquare figures of white wampum; and in alliances, human figures holding a chain, which is their emblem of friendſhip, and each figure repreſents a nation. An axe is alſo ſometimes deſcribed, and always imports war: the taking it up, being a declaration of war; and the burying it, a token of peace.

With reſpect to your queſtions concerning the chief magiſtrate, or Sachem, and how he acquires his authority, &c.; I am to acquaint you, that there is, in every nation, a Sachem, or chief; who appears to have ſome authority over the reſt, and it is greateſt amongſt the moſt diſtant nations. But in moſt of thoſe bordering on our ſettlements, his authority is ſcarcely diſcernible, he ſeldom aſſuming any power before his people. And indeed this humility is judged the beſt policy; for, wanting coercive power, their commands would perhaps occaſion aſſaſſination, which ſometimes happens.

The Sachems of each tribe are uſually choſen in a public aſſembly of the chiefs and warriors, whenever a vacancy happens by death or otherwiſe; they are generally choſen for their ſenſe and

and bravery from among the oldeſt warriors, and approved of by all the tribe; on which they are ſaluted Sachems. There are, however, ſeveral exceptions; for ſome families have a kind of inheritance in the office, and are called to this ſtation in their infancy.

The chief Sachem, by ſome called the King, is ſo either by inheritance or by a kind of tacit conſent, the conſequence of his ſuperior abilities and influence. The duration of his authority depends much on his own wiſdom, the number and conſequence of his relations, and the ſtrength of his particular tribe. But even in thoſe caſes where it deſcends, ſhould the ſucceſſor appear unequal to the taſk, ſome other Sachem is ſure to poſſeſs himſelf of the power and the duties of the office. I ſhould have obſerved, that military ſervices are the chief recommendations to this rank. And it appears pretty clearly, that heretofore the chief of a nation had, in ſome ſmall degree, the authority of a ſovereign. This is now the fact among the moſt remote Indians. But as, ſince the introduction of fire arms, they no longer fight in cloſe bodies, but every man is his own General; I am inclined to think this has contributed to leſſen the power of a chief. This chief of a whole nation has the cuſtody of the belts of wampum, &c. which are as records of public tranſactions: he prompts the ſpeakers at all treaties, and propoſes affairs of conſequence. The chief Sachems form the grand council; and thoſe of each tribe often deliberate apart on the affairs of their particular tribes. All their deliberations

 are

are conducted with extraordinary regularity and decorum. They never interrupt him who is speaking; nor use harsh language, whatever may be their thoughts.

The chiefs assume most authority in the field: but this must be done, even there, with great caution; as a head warrior thinks himself of most consequence in that place.

The Indians believe in, and are much afraid of, witchcraft: those suspected of it are therefore often punished with death. Several nations are equally severe on those guilty of theft, (a crime indeed uncommon among them): but in cases of murder, the relations are left to take what revenge they please. In general, they are unwilling to inflict capital punishments, as these defeat their grand political object, which is, to increase their numbers by all possible means.

On their hunts, as upon all other occasions, they are strict observers of *meum* and *tuum*, and this from principle, holding theft in contempt; so that they are rarely guilty of it, though tempted by articles of much value. Neither do the strong attempt to seize the prey of the weak; and I must do them the justice to say, that, unless heated by liquor or inflamed by revenge, their ideas of right and wrong, and their practices in consequence of them, would, if more known, do them much honor. It is true, that, having been often deceived by us in the purchase of lands, in trade, and other transactions, many of them begin now to act the same part. But this reflects most on those who set them the example.

As

As to your remark on their apparent repugnance to civilization, I muſt obſerve, that this is not owing to any viciouſneſs of their nature, or want of capacity; as they have a ſtrong genius for arts, and uncommon patience. I believe they are put to the Engliſh ſchools too late, and ſent back too ſoon to their people, whoſe political maxim, Spartan-like, is to diſcountenance all purſuits but war, holding all other knowledge as unworthy the dignity of man, and tending to enervate and divert them from that warfare on which they conceive their liberty and happineſs depend. Theſe ſentiments conſtantly inſtilled into the minds of youth, and illuſtrated by examples drawn from the contemptible ſtate of the domeſticated tribes, leave laſting impreſſions; and can hardly be defeated by an ordinary ſchool education.

I wiſh my preſent leiſure would allow me to give you as many ſpecimens of their language as would ſhew, that (though not very wordy) it is extremely emphatical; and their ſtyle adorned with noble images, ſtrong metaphors, and equal in allegory to many of the Eaſtern nations. The article is contained in the noun, by varying the termination; and the adjective is combined into one word. Thus of *Echin*, a man, and *Gowana*, great, is formed *Echingowana*, a great man. *Cahyunghaw* is a creek, *Caghyungha* a river, *Caghyunghaowana* a great river; *Caghyungheeo* a fine river. *Haga* the inhabitants of any place, and *Tierham* the morning; ſo, if they ſpeak of Eaſtern people, they ſay *Tierhanſ-aga*, or people of the morning. *Eſo* is expreſſive of a great quantity,

and *Esogee* is the ſuperlative. The words *Goronta* and *Golota*, which you mention, are not of the ſix nations, but a Southern language. It is curious to obſerve, that they have various modes of ſpeech and phraſes peculiar to each age and ſex, which they ſtrictly obſerve. For inſtance, a man ſays, when he is hungry, *Cadagcariax*, which is expreſſive both of his want and of the animal food he requires to ſupply it; whilſt a child ſays, in the ſame circumſtances, *Cautſcre*, that is, I require ſpoon-meat.

There is ſo remarkable a difference in the language of the ſix nations from all others, as affords ground for enquiring into their diſtinct origin. The nations North of the St. Lawrence, thoſe Weſt of the great lakes with the few who inhabit the ſea-coaſts of New England, and thoſe again who live about the Ohio, notwithſtanding the reſpective diſtances between them, ſpeak a language radically the ſame, and can in general communicate their wants to one another; while the ſix nations, who live in the midſt of them, are incapable of conveying a ſingle idea to their neighbours, nor can they pronounce a word of their language with correctneſs. The letters *M* and *P*, which occur frequently in the other languages, are not in theirs; nor can they pronounce them but with the utmoſt difficulty. There is indeed ſome difference of dialect among the ſix nations themſelves; but this is little more than what is found in all the European ſtates.

Received

Received January 15, 1772.

XVIII. *An Account of ſome curious Fiſhes, ſent from* Hudſon's Bay; *by Mr.* John Reinhold Forſter, *F. R. S. in a Letter to* Thomas Pennant, *Eſq; F. R. S.*

DEAR SIR,

Read Jan. 28, 1773.

THE Governor and Committee of the Hudſon's Bay Company preſented the Royal Society with a choice collection of ſkins of quadrupeds, many fine birds, and ſome fiſh, collected by their ſervants at the ſeveral ports in Hudſon's Bay; the Committee of the Royal Society, for examining and deſcribing theſe curioſities, did me the honour to refer them to me for examination. I wiſh the following obſervations on the fiſh, which I take the liberty to addreſs to you, as a lover of Natural Hiſtory, and my remarks on the birds and quadrupeds, may convey ſuch informations concerning the zoology of North America, in the ſtudy of which you have made ſuch vaſt progreſs; and ſo particularize the animals of the *Arctic* part of that vaſt continent, that nothing further may prevent

your favouring the public with the reſult of your ſtudies on that ſubject.

The four kinds of Hudſon's Bay fiſh are the *Sturgeon*, the *Burbot*, the *Gwiniad*, and a new fiſh called the *Sucker* at Hudſon's Bay.

The Sturgeon was about fourteen inches long, and therefore ſeems to be a young fiſh; as it is likewiſe obſerved in the liſt, written by the Gentleman who ſent this fiſh from York Fort.

DESCRIPTION.

Its noſe is very long and ſlender, terminating in a point; the eyes are ſmall; under the projecting ſnout, before the mouth, are four beards or *cirri*, placed nearly in the ſame line, and not by pairs, as in ſome other ſpecies of Sturgeon. The mouth is beneath, nearly oppoſite the eyes, toothleſs, cartilagineous, ſemilunar when in its natural poſition, but round when open; on each ſide are two noſtrils. The whole head is depreſſed, and very nearly quadrangular; the whole body pentagonal, and tapering towards the tail; the whole ſkin tough, covered with five rows of uncinated ſcales; the dorſal ſeries conſiſts of fourteen large roundiſh ſcales, and a ſingle one behind the dorſal fin; each of the lateral rows has 35 oblique ſcales; in the two ventral rows are nine roundiſh ſtrong ſcales between the pectoral and ventral fins; one ſcale is behind the vent, and ſtill another behind the anal fin.

The fiſh, according to this deſcription, ſeems to come the neareſt to that ſpecies of Sturgeon which I de-

I described in the Philos. Transactions, Vol. LVII. in my *Specimen Historiæ Naturalis Volgensis*, N° 10. under the name of *Acipenser Ruthenus major, rostro elongato acuminato, paululum supino*, and which the Russians call *Sevruga*. Kramer, in his *Elenchus Vegetabilium & Animalium Austriæ*, p. 383. is the only writer that I know who takes notice of this species; he calls it *Acipenser rostro acuto, corpore tuberculis spinosis aspero*: the inhabitants of Austria call it *Shirk*, a name they have no doubt borrowed from the Slavonian name *Sevruga*. The famous painter and traveller Cornelys de Bruyn mentions this kind of fish, but in so superficial a manner, that one plainly sees he was little, if at all, used to discussions in points of Natural History. He says, * "the *Sterlet* is the "best fish in Russia; there are two species of it; "but, upon the whole, it is nearly related to the "Sturgeon. The *Severukas* differ in nothing from "the Sturgeon, which the Russians call *Assetrina*. "The Caviar is made from the *Beluga*, the *Assetrina*, and the *Severuka*." Had de Bruyn examined the *Sevruga*, he would certainly have found it materially different from the *Osetr* or *Assetrina*, i. e. the common blunt-nosed Sturgeon of Germany and the Baltick. I suppose the English Sturgeon, from your own description †, and the drawing in the British Zoology, illustrated by plates, tab. LXXXIX. to be the same with this kind from Hudson's Bay, and with the *Sevruga* of the Russians, and the *Shirk* of the Austrians. The true Sturgeon, which gave

* De Bruyn's Voyage, Tom. I. &c. Amsterd. fol. p. 93.
† Br. Zool. octavo, Vol. III. p. 99.

the

the name to the whole genus *, I think to be an unknown fiſh in England. The ſpecies of Sturgeons are more numerous than one is at firſt aware of; and it would therefore be of ſome utility, that perſons, who have an opportunity of examining all the various kinds at Vienna, and in Ruſſia, might do it with more care than has hitherto been done. Some of the ſorts which I have ſeen, I have ſo deſcribed that they may be known again; ſome I did not ſee, and gave their characters from books, and from the reports of ſuch perſons as had examined them. Mr. Klein, a very ingenious naturaliſt, has enumerated ten Sturgeons, in his 4th *Miſſus Piſcium*, p. 11—16. and Count Marſigli, in his ſplendid work about the Danube, Tom. IV. gives the names of at leaſt ſix Sturgeons, but the characters are not ſufficiently ſettled in both theſe works. Klein ſaw but two kinds of Sturgeons, and a third in ſpirits; and Count Marſigli was not enough of a naturaliſt to give adequate deſcriptions of theſe fiſh. Therefore it is certain that a careful examination and accurate account of the ſeveral ſpecies of Sturgeons would greatly illuſtrate the Natural Hiſtory of this genus.

The ſecond of the Hudſon's Bay fiſh, is called, by the wild natives of that country, *Marthy*, and is nothing elſe than our common Burbot, *Gadus Lota*, Linn. only vaſtly ſuperior in ſize. The deſcriptions

* The Germans call this fiſh *Stoer*, from the old Teutonic word *Stor* or *Stuhr*, which ſignifies *great*, as this fiſh grows to a very large ſize. Thus likewiſe the Scotch call the *Tunny*, Mackrel *Sture*. Vide Mr. Pennant's Tour in Scotland, p. 192.

you

you have given of this fiſh, in the Britiſh Zoology, is entirely correſponding with this ſpecimen, ſo that it would be ſuperfluous to preſume to make any additions to it. I muſt, however, obſerve, that, after a moſt minute examination, I could find no more than ſix branchioſtegous rays in the two ſpecimens from Hudſon's Bay, of which you mention ſeven in the Engliſh Burbot, and Artedi as many in his ſpecimens. This great naturaliſt ſeems likewiſe to be right, when he obſerves that the *cirri*, or beards on the end of the noſe, are the valves to one of the noſtrils; for I found that theſe beards, on their under-ſide, opened into a hole, correſponding with the lower noſtril. Mr. Andrew Graham, the collector of the Natural Hiſtory ſpecimens at Severn River in Hudſon's Bay, obſerves, that theſe fiſh conſtantly ſwim cloſe to the ground, and are extremely voracious; for he repreſents them as not content with devouring every fiſh * they can overcome, but likewiſe feeding on putrefying deer, or other carrion that comes in their way; even ſtones are ſometimes ſwallowed to ſatisfy their inſatiable appetite, of which Mr. Graham was himſelf a witneſs, having taken a ſtone of a pound weight out of the ſtomach of this fiſh. The pike is often obliged to fall a victim, together with the trout, *Tickomeg*, and others, to this rapacious fiſh. After ſunſet, it is caught by a night-hook. It does not maſticate its food before deglutition. Its roe and liver are reckoned a delicacy, when freſh caught; but they turn rancid and

* This too is the fiſh that makes ſuch havock in the Lake of Geneva. P.

 oily

oily in a few days, though kept frozen ſolid all the time. At Hudſon's Bay this fiſh is thought to be dry and inſipid; its weight is from one to eight pounds.

The third ſpecies of fiſh, from this cold climate, is by the natives called *Tickomeg*, and is our *Gwiniad* or *Salmo Lavaretus*, Linn.; only the ſize is ſomewhat bigger, for the greateſt ſpecimen ſent over meaſures 18 inches from the head to the tip of the tail, is $4\frac{1}{2}$ inches deep, and not above an inch and $\frac{1}{4}$ thick. This fiſh differs in no circumſtance from our Gwiniad, but the length. You mentioned in your Britiſh Zoology (Vol. III. p. 269.) a *Ferra* or *Gwiniad* from Switzerland 15 inches long, as an uncommon ſize *; the Hudſon's Bay fiſh, as I have before obſerved, is 18 inches long, and $4\frac{1}{2}$ inches its greateſt depth. The great abundance of food, and the ſmall number of inhabitants, who let the fiſh grow up undiſturbed, are perhaps the cauſes of their uncommon bigneſs. They weigh from $1\frac{1}{2}$ pound to 3 pounds, ſays Mr. Graham; but, I am ſure, the fiſh I examined muſt, when freſh, have weighed more. Theſe fiſh abound in the River Severn in Hudſon's Bay, from its origin in the great lakes to its mouth, where it empties itſelf into the bay. The natives catch five or ſix hundred a day, by means of wears which they contrive in the river: they will not take bait, and are poor at the breaking of the ice in the river. In the middle of the ſummer, after a gale of wind,

* However, the Gwiniads of Lapland, a ſimilar climate to that of the Hudſon's Bay, are vaſtly large. Brit. Zool. III. 267. note.

they are often found thrown up into the marſhes, and on the ſhoals, where they remain at the receſs of the water and abating of the wind, and ſerve as food to numbers of crows. The inhabitants of Hudſon's Bay think this fiſh very ſweet, and good to eat, contrary to the opinion of many Europeans.

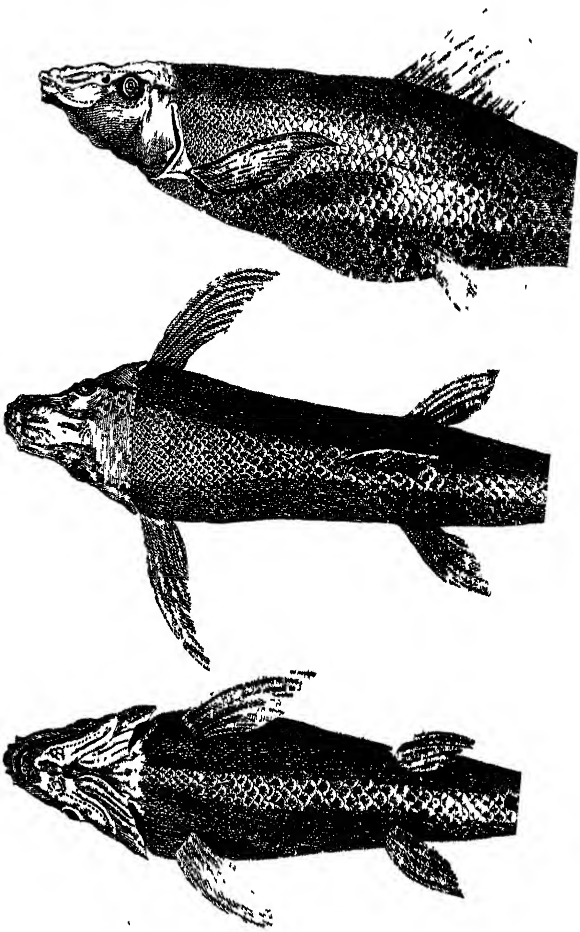
The fourth and laſt fiſh brought from Hudſon's Bay is there called a *Sucker*, becauſe it lives by ſuction, according to Mr. Graham's account, who likewiſe ſays, that there are two varieties of this fiſh, both of a whitiſh colour, but one diſtinguiſhed by a mixture of beautiful red. In the ſmalleſt of two ſpecimens brought over, a broad ſtripe of red could be obſerved all along the *linea lateralis*. They are very numerous in the creeks and rivers, and troubleſome in overburdening the nets. They are not deemed a palatable food, being very ſoft, and full of ſmall bones. They weigh from one half to 2½ pounds.

The above is literally what Mr. Graham ſays of this fiſh, and all that is known of its natural hiſtory. Examining it carefully, I found it was a new ſpecies of the genus of *Cyprinus*, or *Carp*.

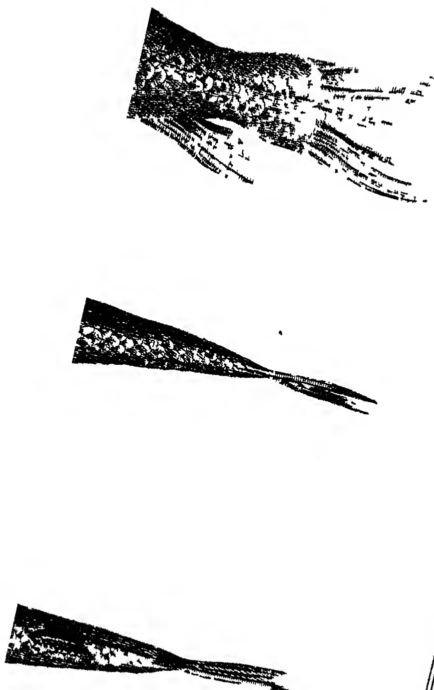
The head is broader than the body, gradually decreaſing towards the noſe, full of elevations and tubercles, nearly quadrangular, and not ſcaly. The mouth is quite under the head, as in the *Loricariæ*, when ſhut, ſemilunar; when open, round; not far from the extremity of the ſnout, and included in ſmall round lips. To the under-lip is fixed a bilobated, beard-like, papilloſe caruncula; it has no teeth. The

eyes are large, but the colour of the iris could not be determined. The number of the branchioftegous rays is three. The body is flat, tapering towards the tail, and fcaly. The greater fpecimen meafures very near 15 inches from the nofe to the extremity of the tail; next to the head it is nearly two inches thick, about the dorfal fin $1\frac{1}{4}$ inch; its greateft depth before the ventral fins is $2\frac{4}{5}$ inches. On the fnout are about five round prominent tubercles; two noftrils are found on each fide, the biggeft next before the eye is kidney-fhaped. The covers of the gills are double, and divided; the head has feveral futures; over each eye, in a cavity, are two longitudinal ones, joined oppofite the noftrils by a ftill fhorter tranfverfe one; on the covers of the gills are two, on each fide one, beginning near the lobes of the caruncula of the under-lip, and going up arched towards the eye. Near the extremity of the fnout begins on each fide a longitudinal one; it paffes under the eye, and mounts in a curvature behind it, then it goes on ftraight to the end of the head, where it again gets downwards, and joins the lateral line. Where the head joins to the body, thefe two futures are connected by a tranfverfal one, which, as it were, feparates the head from the body. The lateral line at firft defcends from the head, but then runs on ftraight, rather nearer the back than the body, to the beginning of the tail. The fcales are fmall near the head and back, increafing in fize towards the middle and tail, clofe to which they are again fmaller. The dorfal fin is placed fomewhat behind the equilibrium of the fifh, rhomboidal, and confifting of twelve ftrong branched rays. The pectoral fins are lanceo-

Cyprinus Catostomus.



r Sucker



lanceolated, fixed under the covers of the gills, and have 17 rays. The ventral fins have 10 or 11 rays, and are placed in the middle of the belly, and under the dorſal fin. The anal fin conſiſts of eight branched ſtrong rays. The tail is ſomewhat forked or concave, and conſiſts of ſeventeen rays.

I have been as circumſtantial as poſſible in deſcribing this new ſpecies, and join here, together with a drawing [See TAB. VI.], a Latin ſcientific deſcription of the ſame.

I am,

With the trueſt regard,

DEAR SIR,

Your moſt obedient,

humble ſervants,

Jn° Reinhold Forſter.

N° 2. Somerſet Stable-yard, Strand,
January 12, 1772.

Κατω infra Στομαος. } CYPRINUS Catoſtomus.

CYPRINUS pinna ani radiis VIII. labio imo caruncula bilobata papilloſa, cauda bifida. Pinnæ D. 12. P. 17. V. 10.—11. A. 8. C. 17.

Habitat in Sinus Hudſonis fluminibus copioſe, ſugendo paſcitur. Anglis, the *Sucker*.

DESCR. *Caput* ſubtetragonum, verſus apicem ſenſim attenuatum, obtuſiuſculum, corpore fere craſſius, & minus latum.

Tubercula globoſa, confertiora in apice roſtri, circiter quinque; carinata & acuminata, in vertice ſparſa.

Foramina (ſive nares) gemina, quorum altérum minus, alterum oculis proximum, majus, reniforme.

Oculi magni, ad marginem ſuperficiei verticalis capitis ſiti, fere in medio inter apicem & baſin. *Irides* . . . . .

Opercula branchiarum magna, nuda; at ſub oculis opercula ſpuria, primo intuitu pro radiis membranæ branchioſtegæ facile ſumenda.

Suturæ in capite plures catenulatæ; una utrinque brevis, ſupra oculos, nareſque, nec baſin nec apicem capitis attingens, è regione narium juncta per ſuturam tranſverſalem breviſſimam; ſecunda utrinque incipiens ad angulum loborum carunculæ,

carunculæ, imo labio adnata, in operculo ſpurio recurvatur, & prope oculos deſinit: tertia utrinque incipiens prope roſtri apicem, linea recta ſub oculis ducta, dein curvatur & aſcendit verſus verticem; ibi rurſus curvatur & jungitur lineæ laterali, pone aperturam branchialem deſcendenti: connectuntur hæ duæ lineæ laterales ſutura tranſverſa, quæ caput à reliquo corpore diſtinguit.

Membrana Branchioſtega, radiis tribus brevibus, validis.

Rictus inferus, lunulatus, ſeu ſemiorbicularis, labiis incluſus tenuibus, ſuperiore (ore ſcilicet clauſo) concavo, inferiore convexo.

Caruncula lata, labio inferiori adnata, craſſiuſcula, carnoſa, papillis tecta, oris angulos ambiens, medio in lobos binos profunde diviſa.

CORPUS lateribus compreſſiuſculum, at verſus abdomen magis compreſſum, cuneiforme, capite ad caudam ſenſim attenuatum, tectum ſquamis minoribus, ovatis, ſtriatis verſus caput minimis, pallide argenteis, in quibuſdam circa lineam lateralem aureo-rubris.

Linea lateralis recta, dorſo parallela, ad caput ſuper aperturam branchialem adſcendens.

Anus.

Anus parvus, caudæ multo proprior quam capiti.

Pinna dorſi pone æquilibrium nonnihil poſita, rhomboidalis, radiis validis, ramoſis duodecim.

Pinnæ pectorales lanceolatæ, infra opercula ſitæ radiis 17 longitudine partem quartam totius piſcis (excluſis capite & caudâ) æquant.

Pinnæ ventrales radiis 10 vel 11 oblongæ, in medio ventris, ſub pinna dorſali poſitæ, pinna pectorali dimidio breviores.

Pinna ani caudæ propinqua, longitudine fere pinnæ pectoralis, radiis octo validis, ramoſis.

Cauda leviter bifurca, pinnam pectoralem longitudine & numero radiorum æquans.

Longitudo totius piſcis unciarum 15 pedis Anglicani.

Latitudo unciarum circiter 3 ante pinnas ventrales.

Craſſities corporis prope caput unciarum fere 2, ante pinnam dorſalem unciæ & quadrantis.

P. S. Beſides the above-mentioned fiſh, the ſervants of the Hudſon's Bay Company have likewiſe ſent over from thence the common *River Crayfiſh* (Cancer Aſtacus, *Linn.*), which, in every particular, correſponds with the Engliſh one.

Received

Received November 12, 1772

XIX. *Experiments upon the different Kinds of Marle found in* Staffordshire, *by* William Withering, *M. D. Communicated by* Charles Morton, *M. D. Sec. R. S.*

Read Feb. 4, 1773.

umbe	Description.	Quantity of calcarious earth in half a dram, as separated by the nitrous acid, and precipitated by mild fixed alkaly.	What was left after the foregoing separation, was no longer acted upon by the nitrous acid; but being		One dram of each of the marles being calcined, weighed			The calcined marles put into water, produced
		Grains.	Mixed with water, became	When burnt	Grains.	Lost grains.	Burnt to	
1.	Red and blue intermixed, in small friable lumps.	1	Uniform and plastic.	A hard red brick.	52		Red brick.	No effect.
2.	Red, in small friable lumps.	$0\frac{3}{4}$	Uniform and plastic.	A hard red brick.	53	7	Red brick.	No effect.
3.	Grey, in large hard lumps.	5	Plastic, but a little gritty.	A soft yellowish brick.	49	11	Soft yellow brick.	Weak lime water.
4.	Red, hard, compact.	3	Uniform and plastic.	A hard red brick.	50	10	Red brick.	No effect.
5.	Red, with grey spots, in large hard lumps, scarcely to be broken with a hammer.	$8\frac{1}{2}$	Plastic.	A soft pale red brick.	48	12	Hard grey stone.	Lime water.
6.	Light grey, like a grit stone.	8	Gritty, no union.	No union.	51	9	Soft and stony.	Lime water.
7.	Brown, friable, in large lumps.	18	No union.	A little cohesion.	46	14	Soft stone.	Lime water.
8.	Red, in large friable lumps.	14	Plastic, but a little gritty.	A soft red brick.	48	12	Soft stone.	Strong lime water.
9.	Brownish white, very hard, like calcarious incrustations.	16	No union, gritty.	No union.	43	17	Soft stone.	Strong lime water.
10.	Lead colour, friable, flaky.	$14\frac{1}{2}$	No union, gritty.	No union.	48	12	Soft stone.	Strong lime water.
11.	Brown grey, very hard, in irregular lumps.	16	No union, gritty.	No union.	40	20	Soft stone.	Strong lime water
12.	Lead colour, in powder and in small hard lumps.	$20\frac{1}{2}$	Uniform and plastic.	A soft whitish brick.	29	31	Powdery.	Strong lime water.

Half a dram of the marles being put into similar glass cups, two drams of nitrous acid being added to each glass, they all effervesced; N° 1 and 2 the least, N° 12 the most. The effervescence having ceased, and six drams of rain water being added to each glass, the liquors were all filtered, and after filtration, changed violet paper to a red colour. To the filtered colours was gradually added mild fixed alkali, sufficient to saturate the acid, and precipitate all the earth it had dissolved. The precipitated earth being washed in rain water, till free from all saline matter, weighed, when dry, as in column the third. Column the fourth shews that, after the separation of the calcarious earth, there remained in N° 1, 2, 4, a red clay; in N° 12 a white clay; in N° 8 a red clay, and a portion of sand; in N° 3 a whitish clay, with a portion of sand; in N° 6, 9, 10, 11. pure sand; and in N° 7. sand, with a small portion of clay. These residuums were all washed with rain water before they were burnt. The precipitated powders being mixed together, 82 grains thereof put into a crucible, and calcined with a strong heat, lost 35 grains in weight. Rain water was poured upon the calx; the next morning there was a pellicle upon the surface of the water; it tasted strongly of lime, and let fall a calcarious earth, upon the addition of mild fixed alkaly. The marles were kept for some weeks in a dry place before they were made use of. They were all got out of marle pits in the neighbourhood of Stafford, except N° 12, which is found near the Duke of Bridgewater's canal, in a powdery form, and when mixed with one fourth part of clay is burnt to quick lime. All the above marles crack and fall to pieces, when exposed to the weather.

The foregoing experiments were undertaken with a view to ascertain how far it would be adviseable to attempt burning the marles of this country into quicklime, for the purposes of agriculture; they may likewise furnish us with some useful hints relative to the kind of marles proper to be used upon different kinds of lands. Perhaps the calcarious earth united with clay, as in N° 1, 2, 4, &c. may be the best for light sandy soil; and N° 6, 9, 10, 11, where the calcarious earth is united with sand, the most eligible where the land is already stiff, and abounding with clay. How far the different quantities of fixable air, or other volatile parts, contained in each of the marles, as shewn by column the fifth, will influence their preference in agriculture, must be left to the experience of the farmer to determine.

Received Nov. 12, 1772.

XX. *A Letter from* Patrick Brydone, *Esq;* *to Sir* John Pringle, *Bart. Pres. R. S.* *containing an Account of a fiery Meteor, seen on the* 10*th of* February *last; and also of some new electrical Experiments. Dated* Eccles, *(in the Shire of* Berwick) 7 July, 1772.

SIR,

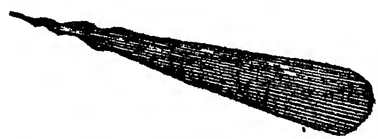
Read Feb. 4, 1773.

ABOUT fourteen years ago, I took the liberty of communicating to you some electrical experiments; which, you thinking worthy of the attention of the Royal Society, were pleased to lay before that respectable body; and those gentlemen did me the honour of publishing them in the first and second part of the fiftieth Volumes of the Philosophical Transactions. Since that time, I have often been engaged in operations of the same kind, some of which, particularly the experiments with the electrical kite, I thought to have troubled you with; but as these were made in foreign countries, and as I was informed that something of the same kind had been done in England, I suspected that you might already

ready have been acquainted with them; and this consideration prevented me. However, last winter, on my return to this kingdom, I observed some facts in electricity, which I flatter myself you will not think undeserving of your notice. What led me to make these experiments, was the strong electrical appearances that the air exhibited during the last great frost, and the observation of several meteors, and other phænomena, that possibly depend on electrical causes. One of these meteors was so remarkable, that I must beg leave to give you some short account of it.

On Monday the 10th of February last, exactly at seven in the evening, as I was riding through Tweedmouth, a village at the south end of Berwick-bridge, I observed that the atmosphere was suddenly illuminated in a very extraordinary manner. The light of the moon, which was about half full, seemed to be extinguished by the blaze; and I saw my shadow projected on the ground, and almost as distinct, and well-defined, as in sunshine. I turned round to see from whence the light proceeded, when I beheld a long, bright flame, moving almost horizontally along the heavens. It was of a conical form, and from the base to the apex could not be less than six or seven degrees; its height, when I first observed it, seemed to be about fifty degrees; but it descended gently, and appeared to burst about five or six degrees lower. Its course was from north-west to south-east, and seemed to have an inclination to the horizon; but this might be only a deception. The base of the cone was rounded like a sphere;

and apparently of about one third of the diameter of the moon at her greateſt height; but its light was brighter than that of the planet Venus, and in colour reſembled the flame of burning camphire. Near the end of the tail there was a kind of waving motion, which with the whole appearance, I have endeavoured to repreſent by this figure ——



In about ten or twelve ſeconds it ſeemed to burſt, dividing into a number of ſmall luminous bodies, like the ſtars in a ſky-rocket, which immediately diſappeared.

As I had formerly obſerved exploſions from meteors of this kind, I had preſence of mind to pull out my watch (which has a ſecond hand) to meaſure the exact time the report ſhould take in reaching me. I waited for upwards of four minutes, which in my ſtate of expectation appeared a much longer time; when, deſpairing of any report, I rode on, but had not got to the middle of the bridge, when I was ſtunned by a loud and heavy exploſion, reſembling the diſcharge of a large mortar, at no great diſtance, and followed by a kind of rumbling noiſe, like that of thunder. I examined my watch, and found, that the ſound had taken five minutes, and about ſeven ſeconds, to reach me; which, according to the common computation of 1142 feet in a ſecond, amounts to the diſtance of at leaſt 66 miles. It did not occur to me to meaſure the duration of the light,

 which

which probably did not exceed ten or twelve ſeconds; and during this ſhort period, the length of the path, the meteor ſeemed to deſcribe, could not be leſs than 30 degrees. I expected to have ſeen ſome account of this phænomenon from Newcaſtle, as, by its direction and diſtance, I imagined it had burſt pretty near to the zenith of that city; but I have found no notice taken of it in the news-papers there. About a week after, I mentioned what I had ſeen to Sir John Paterſon, of Eccles, who told me he was at that time on the road, betwixt Greenlaw and his own houſe, in company with Mr. Thomas Cockburn, of Edinburgh; and, as they were riding to the ſouth, they obſerved the meteor from its firſt appearance, which was about three or four ſeconds ſooner than I had time to turn about and view it; and this, perhaps, is the reaſon that it appeared ſo much higher to them than it did to me. Theſe gentlemen obſerved, that when it firſt became luminous, it was almoſt vertical, but went off deſcending to the ſouth-eaſt, and had in other reſpects the appearance I have deſcribed. They added, that ſome conſiderable time after the light diſappeared, they heard a great report, which they took for a clap of thunder; for the interval was ſo long, that they did not imagine this ſound had any connection with what they had ſeen.

Now, as theſe gentlemen were at leaſt 20 miles to the weſt of the ſpot where I made my obſervation, and as the appearance and height of this meteor ſeems to have been nearly the ſame to them as to me, it is probable that it was at a very

great

great diſtance from the earth, and much beyond the limits that have been aſſigned to our atmoſphere. The ſmaller meteors, which we call falling ſtars, I have frequently obſerved from the mountain of St. Bernard, one of the high Alps; and laſt year I had the good fortune to ſee ſeveral of them from the higheſt region of Mount Etna; an elevation ſtill more conſiderable, and probably the greateſt acceſſible one in Europe, and they always appeared as high, as when ſeen from the loweſt grounds; ſo that probably the height of two or three miles, bears but a ſmall proportion to the common altitude of theſe bodies.

From their frequent appearance, during the laſt froſt, I was inclined to believe, that the air was then in a very favourable ſtate for electrical purpoſes; but not being provided with a common machine, I bethought me of a whimſical one to ſupply the want of it. The back of a cat, it is well known, often exhibits ſtrong marks of electricity; being, therefore, deſirous to try what effect this might produce, when made uſe of inſtead of the glaſs globe, I cut a quantity of harpſichord-wire into ſhort pieces, of five or ſix inches, and tying them together at one end, made the other diverge like the hair of a bruſh. I took a large metal peſtle of a mortar for my conductor, to the end of which I fixed the bruſh of wire; and inſulated the whole, by placing it on a couple of wine-glaſſes. I then took a cat on my knee, and bringing her back under the wires, I began to ſtroak it gently. The animal continued in good humour for a few minutes, and I had the ſatis-

faction to ſee that the conductor was ſo much charged, that it emitted ſparks of a conſiderable force, and attracted ſtrongly ſuch light bodies as were brought near it; but the cat at laſt becoming uneaſy, threatned to put an end to our experiment. The paſſage of the electrical fire, from the hair of her back to the ſmall wires, occaſioned, it ſeems, a diſagreeable ſenſation, which ſhe could not bear; ſo that turning about her head to defend her back, the tip of her ear happening to touch the conductor, and a large ſpark coming from it, ſhe ſprung away in a fright, and would not allow me to come near her more. However, after a long interval, the animal ſeeming to have forgotten her adventure, a young lady in company, leſs obnoxious to her than I was, undertook to manage her. Having firſt covered the back of this lady's hand with a piece of dry ſilk, that none of the electric fire communicated to the wires might be loſt, ſhe then began to ſtroke the cat as I had done, and the conductor ſoon after appeared fully charged: we drew large ſparks from it, and if the animal would have continued quiet, I have no doubt that we ſhould have ſhewn many of the common experiments in electricity; but ſhe ſoon became ſo outrageous, that we were glad to put an end to our operations, without any hopes of being able to repeat them, at leaſt with the ſame inſtrument. In this dilemma I recollected, that a lady had told me, that on combing her hair, in froſty weather, ſhe had often been ſenſible of a little crackling noiſe; and in the dark had ſometimes obſerved ſmall ſparks of fire to iſſue from it. I propoſed, therefore,

therefore, that one of the young ladies would ſuffer the experiment to be made upon her head, which ſhe agreed to. The conductor was then inſulated as before, and the lady having placed herſelf ſo, that the back part of her head almoſt touched the bruſh of wire, I deſired her ſiſter to ſtand behind her, on a cake of bees-wax; who, as ſoon as ſhe began to comb the hair of the former, the conductor emitted ſparks ſtill of a larger ſize than thoſe we had hitherto ſeen. The hair was extremely electric, and when the room was darkened, we could perceive the fire paſs from it along the ſmall wires to the conductor. The young lady who was on the wax, was not a little ſurprized to find, that the moment ſhe began to comb her ſiſter's hair, her own body became electric, darting out ſparks of fire againſt every ſubſtance that approached her. We found, however, that theſe ſparks were not ſtrong enough to fire ſpirits. I then coated a ſmall phial, and ſoon charged it from the conductor; but afterwards I did it more compleatly from the hair itſelf in the following manner. I fixed a bruſh of ſmall wires to the large one that went through the cork of the phial; and taking the phial in my hand, I followed every motion of the comb with the bruſh of wires; and, in the dark, could obſerve the fire paſs by theſe wires into the bottle. In a few minutes I found it was highly charged; when taking a ſpoonful of warm ſpirits in my left hand, and with my right, which graſped the phial, bringing the hook of the great wire near the ſurface of the ſpirits, a large ſpark darted from it,

gave

gave me a ſmart ſhock, and at the ſame time ſet the ſpirits on fire.

The day following, we wanted to repeat our experiments; but as the weather was hazy, and the froſt had greatly abated, they did not ſo well anſwer. However, from making them on ſeveral heads, we found that the ſtronger the hair, the greater was the effect; whereas, ſoft flaxen hair produced little or no fire at all.

It may not be improper to mention, that theſe experiments were made in a warm, dry room, before a good fire, and at a time when the thermometer, in the open air, was at ſix or ſeven degrees below the point of congelation. The hair, which ſucceeded beſt, was perfectly dry, and no powder or pomatum had been uſed on it for ſome months before.

I have the honour to be,

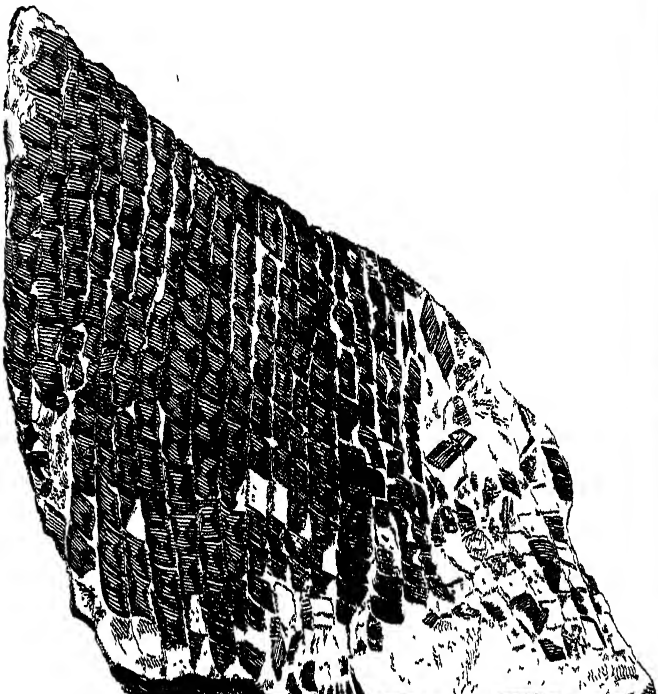
SIR,

Your moſt obedient,

and moſt humble ſervant.

Patrick Brydone.

XXI. *Some*



Basire sc.

XXI. *Some Account of a Foſſil lately found near* Chriſt-Church, *in* Hampſhire; *in a Letter to Dr.* Maty, *Sec. R. S. from the Hon.* Daines Barrington, *Vice-Preſ. R. S.*

DEAR SIR,

Read Feb. 4, 1773. I Send herewith a foſſil which was found in a cliff, near Chriſt-Church, in Hampſhire, by the Rev. Mr. Howel, of Pool, and which Mr. Gough, F. A. S. deſires may be ſhewn to the Royal Society, if it ſhould be thought worthy their attention. [See Tab. VII.]

I am very little verſed in this branch of natural hiſtory; but as I am the unworthy channel of communication, I thought it my duty to procure the beſt information I was able, with regard to this very ſingular ſpecimen.

The ſhining diviſions, upon the ſurface of the ſtone, ſeem to be the ſcales of a fiſh, which I ſhould conceive to be the *Acus maxima ſquamoſa*, engraved in Willoughby's Hiſtory of Fiſh, Tab. p. 8. and deſcribed by Ray, in his Synopſis Piſcium, p. 109.

It appears by the catalogue of Engliſh foſſils, in the collection of Dr. Woodward, that a ſtill larger ſpecimen of the ſame ſort was found in Stansfield quarry, near Woodſtock, though Dr. Woodward could only procure a ſingle ſcale. V. II. p. 53. c. 24.

Single ſcales from the ſame quarry, are alſo to be ſeen in the noble collection of foſſils, given by Mr. Brander, F. R. S. to the Britiſh Muſeum.

Though this fiſh, therefore, is a ſtranger to our ſeas, yet its exuviæ are by no means ſo to our cliffs and quarries.

I am,

Dear SIR,

Your moſt faithful

humble ſervant,

Daines Barrington.

P. S. Mr. Hunter, F. R. S. having ſeen the foſſil at Crane-Court, happened to diſſect a beaver's tail very ſoon afterwards, which he ſhewed me, as bearing a ſtrong reſemblance to the ſcaly diviſions in this ſpecimen; I cannot, however, but ſtill think that the form of the ſcales in the *Acus maxima ſquamoſa* of Willoughby is ſtill nearer to it, than of thoſe in a beaver's tail.

XXII. *A Description of a rare* American *Plant of the* Brownææ *Kind; with some Remarks on this Genus. By Mr.* Peter Jonas Bergius, *F. R. S.*

Read Feb. 11, 1773.

AS the *Leucandendra, Bruniæ, Diosmæ, Phylicæ, Hermanniæ*, &c. are peculiar to Africa, so are likewise the *Varroniæ, Ehretiæ, Samydæ, Malpighiæ, Cacti, Brownææ*, &c. peculiar to America, not having been found in any other country: at present I will confine myself to the last mentioned kind.

Mr. Jacquin, during his botanical travels in America, founded this genus, in memory of Dr. Patrick Browne, the celebrated English botanist; but Jacquin found only one species of this genus; neither was Sir Ch. Linné hitherto acquainted with any more.

I have now specimens of a new species of this kind, which I received from Mr. Pihl, who gathered it in Portobello in America, which will afford an opportunity of exhibiting the whole genus of the *Brownææ*, and the specifical differences of it.

If we compare Mr. Jacquin's description of his species with mine, we see how carefully nature has observed the same order and position of the essential

parts in both; a circumſtance common to all natural genera.

I don't know whether this plant will vegetate and thrive in our ſtoves or green-houſes; if it does, I am convinced it will make a beautiful appearance with its aſſemblage of purple or blood-red flowers.

The drawings which I herewith ſend [TAB. VIII. and IX.], ſhew a ſtalk with a flower, and one without, ſo that the receptacle is bare.

BROWNÆÆ Genus.

1. BROWNÆA (*coccinea*) floribus disjunctis umbellatis.

Brownæa coccinea. Linn. Spec. Plant. 958. Jacquin. Hiſt. Stirp. Americ. 194. * Tab. 121.

Habitat in rupeſtribus ſylvaticis Zaucæ ad ſinum Venezuelæ Americes.

Deſcr. apud Jacquinum, loco citato.

2. BROWNÆA (*Roſa de monte*), floribus aggregato-capitatis ſeſſilibus, ſtaminibus longiſſimis.

Hermeſias. Loefling. Itin. p. 278. *

Habitat in montoſis Terræ Firmæ, Portobello.

DESCR. *Caulis* arboreus.

Rami toruloſi, cortice cinereo.

Ramuli (ſeu petioli communes) ſubalterni, teretes, glabri, baſi geniculo ſuberoſo-rugoſo, patentes.

Folia coriacea, ſpithamæa, oppoſita, ovato-oblonga, integerrima, longius acuta, utrinque glabra, nervis alternis obſoletis, breviter petiolata; inferiora ſenſim minora; infima ovata, baſi ſubcordata.

Petiolelli

Petiolelli breves, crassi, rugosi.

Flores intra calycem communem aggregati in capitulum s. fasciculum subrotundum, speciosissimum, magnitudine pugni.

Fasciculi solitarii, alterni, distantes, sessiles, subaxillares.

Calyx communis imbricatus: foliolis s. bracteis ovatis, acutiusculis, submembranaceis, concavis, laxiusculis, glabris, bipollicaribus, rubicundis; singulis includentibus singulos vel etiam 2 aut 3 flores; deciduis; exterioribus rotundatis; interioribus minoribus, sensim linearibus.

Perianthium proprium cylindricum, tubulatum, superne paululum ampliatum, rubicundum, villosum, bifidum: laciniis ovatis, acutiusculis, subæqualibus, erectis.

Corolla universalis uniformis, sanguinea.

Propria duplex.

Exterior infundibuliformis, calyce longior: tubus cylindricus, subangulatus, deorsum angustatus, subcoriaceus, persistens; limbus 5-fidus (sæpe 4-fidus): laciniis lanceolatis, obtusis, erectis, inæqualibus, unica duplo latiore, deciduis.

Interior pentapetala: petalis ovato-lanceolatis, obtusis, latiusculis, erectis, corolla exteriore fere duplo longioribus: unguibus subulatis, margini tubi corollæ exterioris insertis.

Stam.

Stam. Filamenta conſtanter undecim, filiformia, longiſſima, h. e. corolla duplo longiora, erecta, ſubcurvata, æqualia, inferne coalita in tubum integrum, antice hiantem, germen cingentem, margini tubi corollæ exterioris adnatum, dein in filamenta, baſi æqualia, fiſſum.

Antheræ ovatæ, incumbentes.

Piſtill. Germen ſuperum, è tubo corollæ exterioris pedunculatum: pedunculo tubo adnato; cylindricum, tomentoſum.

Stylus filiformis, longitudine ſtaminum, inflexus.

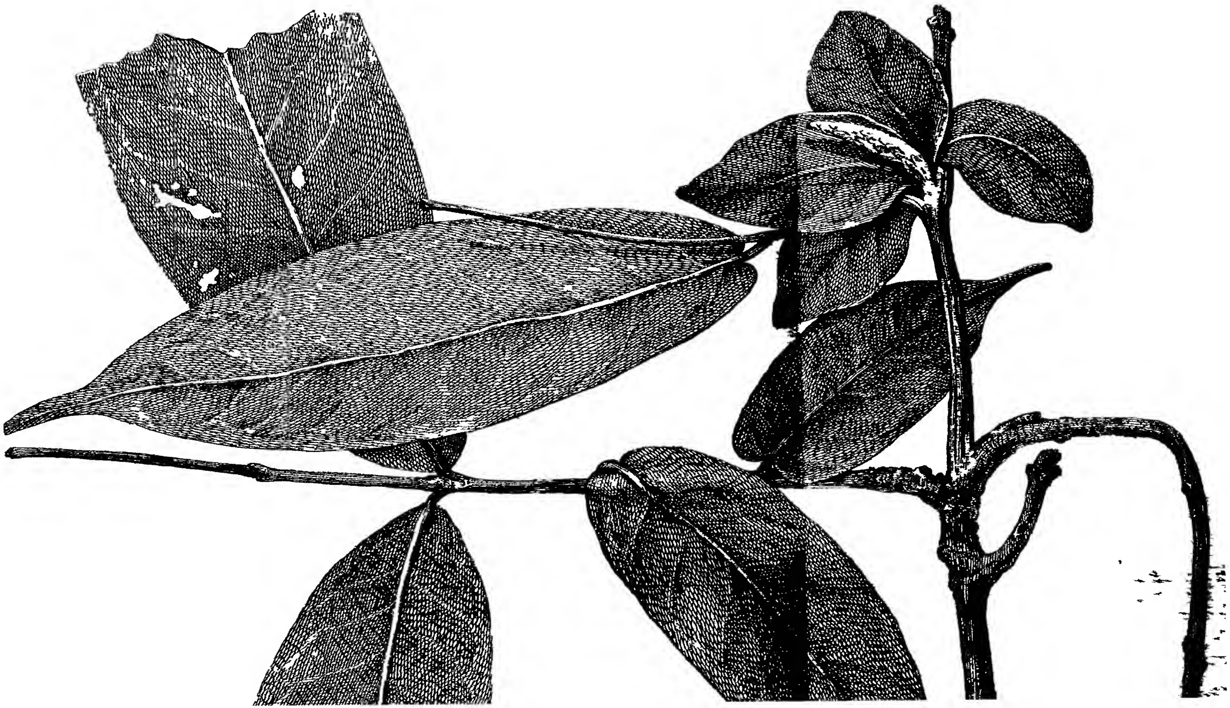
Stigma ſimplex.

Peric. Legumen oblongum, compreſſum, circa diſſepimentum anguſtatum, plerumque biloculare.

Diſſepimento membranaceo.

Semina ſolitaria, ovata, compreſſa, rugoſiuſcula, fibris fungoſis obvoluta.

Bro ae (Rosa de Monte.)



Received Nov. 12, 1772.

XXIII. *Extract of a Letter from the Rev.* Samuel Kirkshaw, *D. D. Vicar of* Leeds, *and Rector of* Ripley, *in the County of* York, *to* William Mountaine, *Esq*; *F. R. S. Communicated by* William Watson, *M. D. and F. R. S.*

Read Feb. 11, 1773.

ON the 29th of Sept. last, about two o'clock in the morning, were three remarkably loud claps of thunder, attended with proportionable lightning. Mr. Thomas Heartly, formerly Wine-merchant, of Leeds, but lately retired from business to Harrowgate, lived there in a hired house, the second northward from the Queen's-head. While he was in bed with his wife, she was awaked from sleep by the thunder, and went to the window; but not being afraid, she got to bed again, and fell asleep. About five she awaked; and, not perceiving her husband to breathe, though warm, endeavoured to awake him—in vain! She quickly sent for Mr. Hutchinson, a considerable apothecary at Knaresborough, who, upon sight of Mr. Heartly, and some experiments, declared him dead, though still very warm. At her request,

requeſt, however, he opened a vein; and Mr. Heartly bled freely, inſomuch that the blood did not ceaſe to ooze out of the orifice till the body was put in the coffin, which was on Thurſday evening, the firſt inſtant, viz. October, and it was not even then cold. His hair, which he wore, was conſiderably burnt, or ſinged on the right ſide of his head, which was uppermoſt (for he lay then on his left ſide) and the inſide of his night-cap, on the ſame ſide, was ſinged or browned, though no where on the outſide marked at all. Within the cap was found a ſplinter from the bed-poſt next to his head, which poſt was torn and ſplit into many ſplinters or ſhivers, from the top to the bottom, though a ſtrong oaken poſt, and almoſt new. No wound, or mark of any ſort, was diſcovered on any part of his body; but the lower part of his right cheek was ſwelled, and much hardened. So far I was told by Mrs. Heartly and her ſervants, but I ſaw the bed-poſt.

In the chamber where this happened, there was a ſmall chimney to the north, made up, but not quite cloſe, by a chimney-board, upon which I could not diſcover any mark or hole, or other indication of the lightning's paſſing that way.

Between that chimney and the weſt end of the room, ſtands the bed, in the north-weſt corner of the room, cloſe to the weſt and north walls; the deceaſed lay next to the weſt wall, with his head near to the head bed-poſt, in the north-weſt corner aboveſaid.

There is only one window in the room, full eaſt, conſiſting of three pretty large lights, ſepa-

rated by two ſtone mullions, each light ſupported by ſix ſtrong iron bars acroſs it, parallel to the floor, and the intermediate one, rather more than half of it, made into a caſement, the frame of which is of iron, and the ſurrounding frame of the ſame. In the ſouthermoſt light, which had three ſquares of glaſs in breadth, two of the loweſt ſquares were perforated in or near the middle, about an inch ſquare; but as ſome ſmall parts of the glaſs were gone, I could only gueſs at the ſize of the holes, nor could diſtinctly eſtimate the ſhape of them, nor form the ſlighteſt conjecture, whether the lightning had made its ingreſs or egreſs through both, or either of them. The intermediate ſquare of glaſs left perfectly ſound—there was no other iron about the window, except the abovementioned—but the curtain-rods of the bed, which ſtood about ten feet from the window, I obſerved, were iron, ſtronger (larger) ſomewhat than uſual.

Mrs. Heartly lay on Mr. Heartly's left hand, when the thunder was, and felt not the leaſt ſtroke from the lightning, or perceived any effects from it, except that her right arm, ſhe found, when ſhe awoke, was ſtunned and benumbed, and a little painful, which continued for a few days, but is now quite well.

I took notice of a pump, which ſtood about ten or eleven feet from the houſe, in nearly a right line from the window abovementioned, whoſe handle (or ſreape, as it is called hereabout) is all of iron, very thick and long, and a ſtrong iron ball for a head to it.

Ripley, Oct. 12, 1772.

XXIV. *Extract of a Letter from* Paul Panton *Esq; of* Plaſwgyn *in* Angleſey, *to the Honourable* Daines Barrington, *V. P. concerning the Increaſe of Population in* Angleſey.

Plaſwgyn in Angleſey, Oct. 3, 1772.

DEAR SIR,

Read Feb. 25, 1773.

I Wiſhed to have ſent you a fuller account of the ſtate of population in this iſland; but ſo little care has been taken to preſerve the pariſh regiſters, that ſcarcely any that are antient are to be met with. Upon the whole of thoſe I ſend you, there is great reaſon to make a pleaſing concluſion, that we become more healthy, and increaſe in population. Heretofore the inhabitants of this iſland lived chiefly upon fiſh, with which, eſpecially herrings, theſe coaſts were abundantly furniſhed. Salted herrings were their principal food. This rambling fiſh, the herring, having left us, our iſlanders have neglected purſuing other branches of the fiſhery, and have betaken themſelves more to agriculture. The potatoe plant has not been cultivated in any great quantities here until of late years; but, ſince the failure of our herring fiſhery, it has made great part of the food of the inha-

inhabitants. Perhaps the want of the one, and the increaſed conſumption of the other, may be amongſt the cauſes that have contributed to the better health of our people. The increaſe in population in Llanduvnan and Pentraeth pariſhes, has not been owing to mines, or any new advantage introduced. The inhabitants are wholly employed in huſbandry.

I am,

Dear SIR,

Your very obliged humble ſervant,

Paul Panton.

BIRTHS and BURIALS, in LLANSADURN Pariſh, Angleſey.

	Chriſtened.	Buried.		Chriſtened.	Buried.
1590	6	2	1620	5	5
91	1	4	21	3	4
92	—	2	22	7	7
93	5	5	23	1	6
94	5	4	24	6	1
95	7	3	25	3	2
96	5	3	26	4	6
97	5	7	27	5	2
	34	30		34	33

BIRTHS, &c. in LLANSADURN Parish.

	Christened.	Buried		Christened.	Buried.
1750	8	6	1764	9	10
51	5	4	65	10	1
52	6	3	66	9	7
53	10	12	67	6	10
54	9	4	68	11	8
55	9	8	69	9	11
56	9	8	1770	10	16
57	9	3	71	5	5
	63	50			

BIRTHS, &c. in PENTRAETH Parish.

	Christened.	Buried		Christened.	Buried.
64	11	14	1672	11	10
1665	7	10	73	8	8
66	6	6	74	10	11
67	6	14	75	13	11
68	8	5	76	10	25
69	8	37	77	19	8
70	7	48	78	19	12
1671	9	43	79	12	21
	69	188		102	106

	Weddings.	Christenings.	Burials		Weddings.	Christenings.	Burials.
1740	2	11	8	1764	5	20	10
41	2	15	17	65	3	22	7
42	7	6	9	66	1	14	9
43	4	14	18	67	5	19	15
44	8	14	10	68	5	18	11
45	2	12	7	69	2	18	15
46	2	14	5	1770	8	14	11
47	5	14	11	71	4	24	12

BIRTHS, &c. in LLANVAIR yn GORNWY Pariſh.

	Chriſtened.	Buried.		Chriſtened.	Buried.
1732—	10	— 13	1764—	12	— 18
33—	7	— 7	65—	8	— 11
34—	12	— 6	66—	16	— 5
35—	9	— 5	67—	10	— 15
36—	6	— 17	68—	10	— 8
37—	10	— 7	69—	19	— 5
38—	5	— 5	1770—	14	— 9
39—	9	— 7	71—	12	— 6
	68	—67		101.	

BIRTHS, &c. in BEAUMARIS Pariſh.

	Chriſtened.	Buried.		Chriſtened.	Buried		Chriſtened.	Buried.
1676—	19	— 19	1710—	18	— 26	1764—	44	— 22
77—	15	— 17	11—	26	— 29	65—	44	— 28
78—	18	— 19	12—	26	— 21	66—	41	— 38
79—	18	— 16	13—	22	— 17	67—	32	— 35
1680—	14	— 24	14—	45	— 24	68—	44	— 28
81—	16	— 23	15—	31	— 36	69—	37	— 47
82—	14	— 25	16—	33	— 21	70—	47	— 22
83—	18	— 31	17—	35	— 38	71—	39	— 29
	135	—174		236	—212		328	—249

BIRTHS, &c. in LLANDDYFNAN Parish.

	Weddings.	Christenings.	Burials.
1547	—	5	4
48	1	4	3
49	—	8	3
1550	1	7	6
51	1	—	6
52	—	3	2
53	4	3	5
54	1	6	10
	8	36	39

	Weddings.	Christenings.	Burials.
1620	3	2	2
21	2	4	1
22	1	1	7
23	3	6	21
24	1	4	18
25	4	7	8
26	4	12	5
27	2	8	5
	20	44	67

	Weddings.	Christenings.	Burials.
1750	1	7	5
51	1	9	4
52	9	15	6
53	6	23	11
54	4	9	11
55	2	18	4
56	2	14	2
57	3	16	3
	28	111	46

	Weddings.	Christenings.	Burials.
1764	7	18	14
65	5	19	10
66	3	26	14
67	2	17	17
68	2	20	13
69	7	24	24
1770	4	16	12
71	2	14	4
	32	154	108

XXV. *A Letter to the Rev.* Nevil Maskelyne, *F. R. S. Astronomer Royal, from Mr.* Bailly, *of the Royal Academy of Sciences at* Paris: *Containing a Proposal of some new Methods of improving the Theory of* Jupiter's *Satellites. Translated from the* French, *with the Original underneath.*

SIR,

Read Feb. 18 and 25, 1773.

THOUGH I have not the honour of being personally known to you, I flatter myself, you will excuse the liberty I have taken, of communicating to you two methods, of my invention, for perfecting the theory of the satellites of Jupiter. The former of these methods serves to measure their diameters, and the latter is intended to make the observations comparable with each other, though made in different places, and with different instruments. As my intention has been

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Monsieur,

Quoique je n'aye pas l'honneur d'être connu de vous, je pense, que vous ne trouverez pas mauvais, que je prenne la liberte de vous communiquer deux méthodes, que j'ai imaginées, pour perfectionner la theorie des satellites de Jupiter. La première de ces méthodes sert à mesurer leur diametre, et la seconde a pour objet de rendre les observations comparables, soit quelles aient été faites en differens lieux et avec differens instruments. Mon

been to make these methods useful, I cannot do better, than submit them to the best judges. Your reputation, Sir, engages me, to lay them before you, and to beg you would examine them: flattered as I should be with your approbation, I shall receive, with great gratitude, whatever criticisms they may happen to suggest. Observation must determine, whether, by this method, it will be possible to arrive at an agreement, which hitherto has been wished for, without being attained.

You know, that the observations of the eclipses of the third and fourth satellites, made by different observers, vary from each other 3, 4, and 5 minutes, and sometimes more; and that there is even a pretty sensible difference in those of the second. In the 38th page of the preface to my *Essay on the Theory of the Satellites*, which has been presented to the Royal Society, I mentioned the inequality discovered by Mr. de Fouchy, and I suggested, that the perfecting

Mon bût a été que ces méthodes soient utiles; je ne puis mieux faire, que de les soumettre aux meilleurs juges. Votre réputation, Monsieur, m'engage à vous les détailler, et à vous prier de les examiner: votre suffrage me flatterait infiniment, mais je recevrai toujours avec reconnoissance les reflections critiques que ces méthodes pourront vous suggérer. Les observations vous feront connoitre si par cette méthode on peut parvenir a un accord que l'on a souhaité jusques ici sans l'avoir obtenu.

Vous savez, que les éclipses du troisième et du quatrième satellite, faites par differens observateurs, different entr'elles de 3, 4, 5 minutes et quelquefois plus; et que celles du second different même assez sensiblement. Dans *L'Essai sur la Théorie des Satellites*, dont j'ai fait hommage a la Societé royale, je parlais dans la préface, page xxxviii, de l'inégalité découverte par Mr. de Fouchy, et j'annonçais,

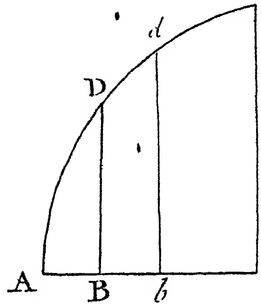
ing of this theory might perhaps depend upon the quantity of this inequality, which Mr. de Fouchy has not determined, not having been at leisure, to resume the subject, since the year 1732. The segment of the disc, which is not eclipsed, when the satellite disappears, must vary in the proportion of the squares of the distances of Jupiter from the Sun, and from the Earth. This is what a little reflection will make evident to every one, and this is the first cause of the inequality. Since Mr. de Fouchy's observation, it has been discovered, that the light of the satellite likewise decreases, in proportion to the proximity of Jupiter's disc; the brightness of the planet weakens that of the satellite, and, for this reason, the eclipses, which happen too near the opposition [of Jupiter to the Sun], are looked upon as defective. Besides, the light of Jupiter, as well as that of his satellites, is different, in his different

nonçais, que la perfection de cette théorie dependrait peut être de la quantité de cette inégalité, que Mr. de Fouchy n'a point déterminée; il n'a pû s'en occuper depuis 1732. Il résulte de la consideration du segment du disque, qui n'est pas éclipsé, quand le satellite disparait, que ce segment varie en raison [*inverse*] des quarrés de la distance de Jupiter au Soleil, et de la distance de Jupiter à la Terre. Voila la premiere cause d'inégalité. Depuis Mr. de Fouchy, on s'est apperçu que la lumiere du satellite diminuoit encore en raison de la proximité du disque de Jupiter; l'éclat de la planete efface celui du satellite, c'est pour cette raison que les éclipses, qui arrivent trop près de l'opposition, sont regardées comme défectueuses. De plus, la hauteur de Jupiter sur l'horison contribuë aussi à affoiblir sa lumiere, et celle de ses satellites;

different elevations above the horizon: when the planet is low, more rays of light are loſt, in their paſſage through a thicker atmoſphere; and whenever the light is leſs, the ſegment, which is not eclipſed when the ſatellite diſappears, and which I call the inſenſible ſegment, increaſes, and occaſions another inequality in the moment of the eclipſes; laſtly, the power of the teleſcopes, or their aperture, which, according as it is greater or leſs, gives more or leſs light, contributes to the variation of this ſegment. Here then are four cauſes of inequality, which I reduce to one principle, and the following is the ſcope of my reſearches. When the ſatellite diſappears, there is certainly a ſegment of its diſc which remains uneclipſed; the magnitude of this varies, on account of the four cauſes juſt mentioned; thence it follows, that

tellites: quand cette planete eſt peu élevée, il ſe perd plus de raions en traverſant une atmoſphere plus épaiſſe; or, toutes les fois que la lumiere diminuë, le ſegment, qui n'eſt pas éclipſé quand le ſatellite diſparait, ce ſegment que j'appelle le ſegment inſenſible, augmente, et il en réſulte une autre inégalité dans le moment des éclipſes; la force des lunettes, ou leur ouverture qui, lors qu'elle eſt plus ou moins grande, fournit plus ou moins de lumiere, contribue encore à faire varier ce ſegment. Voila donc quatre cauſes d'inégalités, que je ramene à un même principe, et quant à mes recherches, voici ce dont il eſt queſtion. Quand le ſatellite diſparait, il y a certainement un ſegment de ſon diſque qui n'eſt pas éclipſé. La grandeur de ce ſegment varie en raiſon des quatre cauſes que nous venons d'établir. D'où il réſulte, que

that if in one eclipse the segment is A B D, and in another A b d, when the satellite disappears in the second eclipse, it will have got less into the shade, by a part of its diameter B b; which part B b, therefore, must be the value of the equation between the two eclipses. Now, if we call A b, a; A B, b, the radius of the disc of the satellite r, the semidiameter of the shadow taken from the tables R, and the total duration of the eclipse d, the time taken up in going over Bb, or the equation ([a]), will be $\frac{2Rr(a-b)}{d}$, which contains three unknown quantities, to wit, the versed sines a and b, of the two invisible segments, and the semidiameter of the disc of the satellite:



que si pour une éclipse ce segment est ABD, et que pour une autre éclipse il soit Abd, dans cette seconde éclipse, quand le satellite disparaitra, il sera moins entré dans l'ombre, d'une partie B b de son diametre, laquelle partie Bb est la valeur de l'équation entre ces deux éclipses. On trouve facilement que si l'on nomme A b, a; A B, b, le raion du disque du satellite r, le demi diametre de l'ombre tiré des tables R, et la durée entiere de l'éclipse d; le tems emploïé à parcourir Bb, ou l'équation, sera $\frac{2Rr(a-b)}{d}$. Cette expression contient trois inconnuës, les sinus verses a et b des deux segmens invisibles, et le demi diametre r du disque du satellite. Car vous savez,

 Monsieur,

tellite: for you know, Sir, that there is nothing to be depended on, in all that has been done upon the diameters of the ſatellites by Caſſini, Whiſton, and Maraldi. The following is the way which I have taken, to determine theſe unknown quantities. I obſerve, firſt of all, that two of them, *a* and *b*, are reducible to one; becauſe, as you will ſee preſently, the two ſegments are always in a known proportion [to the whole diſc of the ſatellite, as well as to each other]; and conſequently, the proportion of their verſed ſines, A*b*, AB, may be obtained, either by calculation, or by a table made for the purpoſe. In order to diſcover it, conſidering that when the ſatellite diſappears, it is from the diminution of its light, I conceived, that one might contrive to imitate, at any time, what happens in the eclipſes, by diminiſhing the light. I have an achromatic teleſcope of five feet length, and 24 lines aperture. I made ſome diaphragms of paſteboard, which I could apply on the

Monſieur, tout ce qui a été fait ſur les diametres des ſatellites par Caſſini, Whiſton, et Mr. Maraldi, et vous ſavez qu'il n'y a rien ſur quoi on puiſſe compter. Voici comment je ſuis parvenu à les déterminer. Je remarque d'abord que les deux inconnuës, *a*, *b*, ſe réduiſent à une ſeule, parceque, comme vous lè verrez tout à l'heure, les deux ſegmens ſont toujours dans un rapport connu, et conſéquemment par le calcul ou par une table dreſſée exprès, on a le rapport de leurs ſinus verſes A*b*, A B. Pour les connaitre, j'ai vû que quand le ſatellite diſparait, c'eſt par la diminution de ſa lumiere, et j'ai penſé, qu'à tous les momens on pouvait imiter, d'une maniere factice, ce qui arrive dans les éclipſes, en diminuant la lumiere du ſatellite. J'ai une lunette achromatique de 5 pieds, qui porte 24 lignes d'ouverture. J'ai taillé des diaphragmes de carton, que je pouvais appliquer

the outſide of my object-glaſs, the openings of which leſſened, by half-lines ſucceſſively, from twenty-four lines down to three. In fine weather, I applied theſe ſuceſſively to my object-glaſs, and endeavoured to find out, whether, by trying from the greateſt to the leſs, ſome one of them could not be found, that would make the ſatellite diſappear. My ſucceſs in this gave me great ſatisfaction. One day, for inſtance, the third ſatellite diſappeared, when the opening was reduced to three lines, and the firſt, when it was reduced to ſix only; and as, in the teleſcopes, the quantity of light is in the proportion of the ſquares of the apertures, I concluded, that the 64th part of the light of the third ſatellite, and the 16th of the firſt, were inſenſible; whence it follows, that if, at the inſtant of an eclipſe of the firſt ſatellite, the ſixteenth part of its light is inſenſible, the inviſible ſegment ABD will be likewiſe a ſixteenth part of the diſc; and thence it will be eaſy to compute

appliquer extérieurement ſur mon objectif, et dont les ouvertures diminuaient de demie ligne, depuis 24 lignes juſqu'à 3. Par un beau tems, j'ai placé ſucceſſivement ſur mon objectif tous ces diaphragmes, et j'ai cherché, ſi en paſſant toujours des plus grands aux plus petits, il n'y en aurait pas quelqu'un, qui fit diſparaitre le ſatellite; et j'ai reuſſi avec une grande ſatisfaction. Un certain jour, par exemple, le troiſième ſatellite diſparut, lorſque l'ouverture fut réduite à trois lignes, et le premier, lorſque l'ouverture fut réduite à ſix ſeulement. Comme dans les lunettes la lumiere eſt dans le rapport du quarré des ouvertures, j'en ai conclu que la 64^{me} partie de la lumiere du troiſième, et la ſeizième du premier étaient inſenſibles : d'où il reſulte, que ſi dans le moment d'une éclipſe du premier ſatellite la ſeizième partie de ſa lumiere eſt inſenſible, le ſegment inviſible ABD ſera auſſi la ſeizième partie du diſque, d'où il eſt aiſé de calculer le ſinus verſe

compute the verſed ſine A B. In theſe firſt obſervations, I took care to chuſe the time, when the ſatellite was at its greateſt elongation; for the inſenſible part increaſes prodigiouſly, and ſometimes amounts to a third of the diſc, when the ſatellite is very near the edge of Jupiter. This variation is much larger, than that which takes place, in conſequence of the diſtance of Jupiter from the oppoſition to the Sun, and contrary to it. As it is ſcarce poſſible to eſtimate the law of the variations of this ſegment, occaſioned by the proximity of the diſc of Jupiter, I judged, that they ought to be determined by obſervation. Accordingly, I followed the ſatellite from the edge of the diſc of Jupiter, to the fartheſt limit of its eclipſes, that is, with reſpect to the firſt, to the diſtance of two ſemidiameters of Jupiter. Having thus ſeveral points by obſervation, I got the reſt by interpolation, and made a table of the variations of the inviſible ſegment,

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verſe A B. J'avais eu ſoin dans ces premieres obſervations, de choiſir le tems où le ſatellite était dans ſes plus grandes digreſſions. Car cette partie inſenſible augmente prodigieuſement, et devient quelquefois le tiers du diſque, lorſque le ſatellite eſt fort près du bord de Jupiter. Cette variation eſt contraire et beaucoup plus grande que celle qui a lieu en vertu de la diſtance à l'oppoſition. Comme on ne peut gueres eſtimer la loi que ſuivent les variations de ce ſegment, à raiſon de ſa proximité du diſque de Jupiter, j'ai jugé qu'il falloit déterminer ces variations par obſervation. J'ai donc ſuivi le ſatellite depuis le bord du diſque de Jupiter, juſqu'au terme le plus éloigné de ſes éclipſes, c'eſt à dire, à l'égard du premier, juſqu'environ à la diſtance de deux demi diametres de Jupiter. Ayant obtenu ainſi pluſieurs points donnés par obſervation, j'ai eu les autres par interpolation, et j'ai dreſſé une table des variations du ſegment inviſible, qui dépendent de la diſtance au bord de

ment, which depend upon the diſtance from the edge of Jupiter; a ſimilar table I likewiſe, made for each of the three firſt ſatellites; but have not yet been able, to make ſufficient obſervations on the fourth. Theſe tables are contained in a long paper of mine, which will be publiſhed in the volume of our Academy for 1771; but, if you pleaſe, I will ſend them to you. Theſe ſegments being known, it is clear, that, beſides their variations occaſioned by the diſtance of the ſatellite from the edge of Jupiter, they will be liable to others. Firſt, In conſequence of the change of Jupiter's diſtances, both from the Sun and from the Earth. Upon this account, the magnitudes of theſe ſegments being known, for a particular epoch *, thoſe known magnitudes muſt be multiplied by $\frac{m^2 n^2}{p^2 q^2}$, to determine the magnitude of the ſegment at any other time. In which expreſſion, p and q ſtand for the diſtances

* *Known*, by the author's tables, for any diſtance of the ſatellite from the edge of Jupiter, at the particular epoch to which the tables are adapted.

de Jupiter; j'ai dreſſé une pareille table pour chacun des trois premiers ſatellites; je n'ai pû obſerver encore, comme il faut, le quatrième. Si vous deſirez ces tables, Monſieur, je vous les envoyerai. Elles ſont dans un long mémoire, qui ſera imprimé parmi ceux de l'Academie pour 1771. Ces ſegmens étant une fois connus, il eſt clair, qu'outre les variations que ces ſegmens ſubiſſent, en vertu de la diſtance du ſatellite au bord de Jupiter, ils varieront encore. 1°. En vertu de la diſtance de Jupiter au Soleil, et de Jupiter à la Terre. Il faudra donc multiplier ces ſegmens, connus pour une certaine époque, par $\frac{mm\,nn}{pp\,qq}$, en ſuppoſant que p et q aient été les diſtances

ſtances of Jupiter from the Sun and from the Earth reſpectively, at the given epoch, and *m* and *n* for the diſtances, at the other time, for which the value of the inviſible ſegment is required. 2dly, There will be other variations depending on Jupiter's height above the horizon. The ſegments which I have obſerved, have all been reduced to the conſtant height of 15°. Mr. Bouguer, in his Optics, has given a table of the degrees of light of the planets, at their different elevations above the horizon, which, from my own obſervations, I have found to be very exact, and uſeful for the preſent purpoſe. Now, as the ſegments are in the inverſe ratio of the numbers of this table, putting *g* for the number correſponding to the elevation of 15°, and *h* for the number correſponding to any other elevation, the ſegments muſt be multiplied by $\frac{g}{h}$. 3dly, Theſe ſegments will yet be ſubject to another variation depending on the aperture of the teleſcope. It is certain that a larger

ſtances de Jupiter au Soleil et à la Terre, au moment de cette époque, et *m*, *n*, les diſtances de Jupiter au Soleil et à la Terre, au moment pour lequel on voudra connaitre le ſegment inviſible. 2. En vertu de la hauteur de Jupiter ſur l'horiſon. Les ſegmens, que j'ai obſervés, ont été tous réduits à la hauteur conſtante de 15°. Mr. Bouguer a donné, dans ſon Optique, une table des degrés de la lumiere des planetes, à differentes hauteurs ſur l'horiſon; je me ſuis aſſuré, par des obſervations, qu'elle eſt très exacte, et très utile dans la recherche préſente. Les ſegmens ſont en raiſon inverſe des nombres de cette table: ſoit donc pour 15° ce nombre *g*, et pour une hauteur quelconque ſoit ce nombre *h*, il faudra multiplier les ſegmens par $\frac{g}{h}$. 3. En vertu de l'ouverture de la lunette. Il eſt certain qu'une ouverture plus grande donnant

larger aperture giving more light, the insensible part of the disk must be smaller; and it seems demonstrable by theory, that this insensible part, or the invisible segments, must be inversely as the squares of the apertures. I resolved, however, to assure myself of this by experiment. For this purpose, I carried my telescope to Mr. Messier's observatory, who has one of Dollond's telescopes, of three feet and a half length, and forty lines aperture. On the 20th of August 1771, he saw the second satellite disappear in his telescope, through an aperture of three lines. The same satellite disappeared in mine, when the aperture was reduced to the same quantity of three lines, and not before. We changed instruments, and, repeating the experiment, found the same effect. Now, the insensible part was $\frac{9}{1600}$ of the disc, in Mr. Messier's instrument, and $\frac{9}{576}$ in mine. These portions, therefore, in these telescopes, were in the inverse proportion of the squares of the apertures.

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donnant plus de lumiere, la partie insensible du disque doit être plus petite, et il parait démontré, par la theorie, que, dans différentes lunettes, cette partie insensible, ou les segmens invisibles doivent être en raison inverse des quarrés des ouvertures; mais j'ai voulu m'en assurer par l'expérience. Je me suis transporté chez Mr. Messier avec ma lunette; il en a une de Mr. Dollond, de trois pieds et demi, qui porte 40 lignes d'ouverture. Le 20 Août 1771, il a vû disparaitre, dans sa lunette, le second satellite, par une ouverture de 3 lignes. Le même satellite n'a disparu, dans la mienne, que par une ouverture également de 3 lignes. Nous avons repété la même expérience, en troquant d'instrumens, et nous avons trouvé les mêmes résultats. Or, dans la lunette de Mr. Messier, la portion insensible était $\frac{9}{1600}$me du disque, et dans la mienne $\frac{9}{576}$. Ces portions, dans ces lunettes, étaient donc en raison inverse des quarrés des ouvertures.

apertures. Confequently, in order to determine the fegments for an aperture of any number of lines k, the fegments of my table, which are all calculated for an aperture of 24 lines, muft be multiplyed by $\frac{576}{k^2}$. Hence, to compute the invifible fegment for any particular eclipfe, the actual diftance of the fatellite from the edge of Jupiter being known, look for the quantity of the invifible fegment, which anfwers to that diftance, in my table, and multiply this quantity by $\frac{m^2 n^2}{p^2 q^2} \times \frac{g}{b} \times \frac{576}{k^2}$. If two different obfervations made in the fame place, or rather two obfervations, made in different places by different obfervers, are to be compared, the invifible fegment muft be determined, fuch as it was for each obferver; A B and A b, the verfed fines of thefe fegments,

ainfi, mes fegmens ayant é é déterminés pour une ouverture de 24 lignes, fi on veut les avoir, pour une lunette, dont l'ouverture aura un nombre de lignes exprimé par k, il faudra multiplier ces fegmens par $\frac{576}{k^2}$. Donc, quand on voudra calculer le fegment invifible, qui aura lieu pour le moment d'une certaine éclipfe, au moïen de la diftance actuelle du fatellite au bord de Jupiter, on cherchera, dans mes tables, le fegment invifible, qui y répond, et on le multipliera par $\frac{m^2 n^2}{p^2 q^2} \times \frac{g}{b} \times \frac{576}{k^2}$. Si on veut comparer enfemble deux obfervations différentes, ou plûtôt, fi on veut comparer deux obfervations faites en différens lieux par deux obfervateurs différens, on déterminera le fegment invifible, qui a lieu pour chacun de ces obfervateurs: on calculera les finus verfes A b et A B de ces deux fegmens, et dans l'expreffion

$2Rr(a-b)$

ſegments, muſt be computed, and in the expreſſion $\frac{2 R r (a - b)}{d}$, the only remaining unknown quantity, will be *r*. The following is the method I have hit upon for determining it. I conſidered, that, by trying different diaphragms ſucceſſively, ſome few minutes before an immerſion, it would be eaſy to find out the particular ſize, which would make the ſatellite diſappear; and that the proportion of the inviſible ſegment to the whole diſc of the ſatellite, for that inſtant, would, by that means, be determined. Suppoſe then that I have found this diaphragm: my next ſtep is, to cover the object-glaſs of my teleſcope with a diaphragm ſomewhat larger, which ſuffers me juſt to perceive the ſatellite, but ſo weak and ſmall, that the leaſt further diminution of its light muſt render it inviſible. I wait till it actually diſappears; I write down this time, then take away the diaphragm, and the number of ſeconds, which paſs betwixt this firſt diſappearance and the

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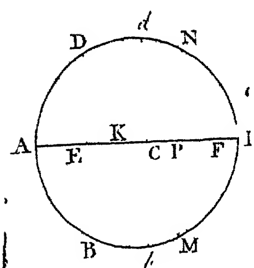
$\frac{2 R r (a - b)}{d}$, on n'aura plus d'autre inconnue que *r*. Voici la méthode que j'ai imaginée pour la déterminer. J'ai penſé, que ſi quelques minutes avant une immerſion, on eſſaiait differens diaphragmes, juſqu'à ce qu'on ait trouvé celui, qui fait diſparaitre le ſatellite, on aurait, pour ce moment, le rapport du ſegment inviſible au diſque entier du ſatellite. Ce diaphragme étant connu, je couvre enſuite l'objéctil de ma lunette d'un diaphragme un peu plus grand, qui me laiſſe appercevoir le ſatellite, mais faible et très petit, de maniere que ce ſatellite ceſſera d'être viſible dès que ſa lumiere ſera tant ſoit peu diminuée. J'attends qu'il diſparaiſſe, [*et je ſuis ainſi averti du moment ou il commence à toucher l'ombre*] (*b*) je marque cet inſtant, j'enleve le diaphragme, et le nombre de ſecondes écoulé, entre cette premiere diſparition et

the true immersion, giving me a great part of the diameter, I easily compute the whole. The following is an example of my method. On the 26th of June 1771, there was an immersion of the third satellite, at 56′ after nine in the evening. I found the diaphragm, which made the satellite disappear, to be of 12 lines. Then I fitted my glass with a diaphragm of 17 lines; I might have taken one much smaller: presently the satellite disappears. But removing the diaphragm, I see the satellite again, very distinctly, for 2′ 18″; after which, the true immersion followed. Now, this is my calculation. The aperture of the diaphragm, which made the satellite disappear, being 12 lines by observation, the invisible segment at the instant of the eclipse, must have been a quarter of the disc. Let ABD be this quarter. I know that, at the instant of the immersion, the satellite had entered the shade, by the whole part EF of its diameter. I say then, if on an aperture of 24 lines, the part ABD is insensible, the insensible part, on an aper-



et la véritable immersion, me donne la mesure d'une grande partie du diametre, d'où il est aisé de conclure le diametre entier. Voici un exemple de la méthode. Le 26 Juin, 1771, à 9 h. 56′ du soir, il y avait une immersion du troisieme satellite. J'ai déterminé de 12 lignes le diaphragme qui faisait disparaitre le satellite. J'ai garni ma lunette d'un diaphragme de 17 lignes (je l'aurais pû prendre beaucoup moins ouvert) j'ai vû disparaitre le satellite, j'ai ôté le diaphragme, j'ai revû, très distinctement, le satellite pendant 2′ 18″, après quoi s'est faite la véritable immersion. Voici le calcul. J'apprends, par l'observation du diaphragme

aperture of 17 lines, will be larger than A B D, in the proportion of the ſquare of 24 to the ſquare of 17. Saying, then as $17^2 : 24^2 :: 0,25000 :: x$, x comes out $= 0,49827$, or near half the diſc repreſented by unity; thence I ſee that, at the inſtant of the firſt diſappearance, the ſatellite had not gone in farther than K. Putting the radius AC $=1$, the verſed ſines AE, AK will be $=0,59602$ and $0,99884$; conſequently, EK $= 0,40282$. Subſtitute this value of E K, inſtead of $a - b$, in the expreſſion $\frac{2R \times r(a-b)}{d}$, and you will have $\frac{2Rr \times 0,40282}{d} = 2' \, 18''$, and $r = 5'23''$ of time, will be the ſemidiameter of the ſatellite, or, the whole diameter will be $22' \, 34''$ [of the ſatellite's orbit, conſidered as a circle, or, would be ſeen, under an angle of this quantity, from Jupiter's centre]. In this obſervation, as I have already

phragme de 12 lignes, qu'au moment de l'eclipſe le ſegment inviſible a dû être le quart du diſque : ſoit A B D le quart du diſque; je ſuis ſûr, qu'au moment de l'immerſion, le ſatellite était entré dans l'ombre, de toute la partie EF de ſon diametre. Je dis enſuite, ſi pour une ouverture de 24 lignes la partie ABD eſt inſenſible, cette partie doit devenir plus grande pour une ouverture de 17 lignes; et elle doit augmenter dans le rapport du quarré de 17 au quarré de 24: faiſant donc comme $17^2 : 24^2 :: 0,25000 : x$ on trouve $x = 0,49827$, ou près de la moitié du diſque, repreſenté par l'unité. Je vois donc, qu'au moment de la premiere diſparition le ſatellite n'était entré encore que juſqu'en K. En ſuppoſant le raïon AC $= 1$, on trouve les ſinus verſes AE, AK égaux à $0,59602$, et $0,99884$: par conſequent, EK $=0,40282$. Si l'on ſubſtituë, pour $a - b$, cette valeur de EK, dans la formule $\frac{2Rr\,(a-b)}{d}$, on aura $\frac{2Rr \times 0,40282}{d} = 2' \, 18''$: d'où l'on tire $r = 5' \, 23''$ de tems, pour le demi diametre du ſatellite, ou $22' \, 34''$ de degrés, pour le diametre entier. Dans cette obſervation, comme je l'ai déja remarqué,

ready ſaid, I uſed a diaphragm with too great an opening (c), for the firſt diſappearance. Take then a ſecond obſervation. On the firſt of Auguſt 1771, there was an emerſion of the third ſatellite, about 15′ after nine. I marked the inſtant of this emerſion; then I furniſhed my teleſcope with a diaphragm of 8 lines. The ſatellite diſappeared, and did not begin to appear again till at the end of 6′ 24″. Some minutes after, when it was quite come out of the ſhadow, I meaſured the diaphragm, which would make it diſappear, and found it of 7 lines. Theſe 7 lines give a ſegment ABD of 0,08507. Then ſaying $8^2 : 24^2 :: 0,08507 : x$; x, or the ſegment ANM, comes out $= 0,76562$, AE $= 0,27994$, AP $=1,43098$, and EP $= 1,15104$. Therefore $\frac{2Rr \times 1,15104}{d} = 6' \, 24''$. From this equation r comes out, 5′ 20″, and the whole diameter 22′22″, [in parts of the circular orbit]. Theſe two concluſions agree ſo

marqué, j'avais emploïé un diaphragme d'une trop grande ouverture, pour la premiere diſparition. Voici une ſeconde obſervation: Le 1 Août 1771, à 9 h. 15′, il y avait une émerſion du troiſieme. Je marquai l'inſtant de cette émerſion, enſuite je garnis ma lunette d'un diaphragme de 8 lignes: le ſatellite diſparut, et il ne commença à reparaitre qu'au bout de 6′ 24″; quelques minutes après, étant tout à fait ſorti de l'ombre, je meſurai le diaphragme qui le faiſait diſparaitre, il ſe trouva de 7 lignes. 7 lignes donnent un ſegment ABD de 0,08507. Faiſant enſuite $8^2 : 24^2 :: 0,08507 : x$, on trouve x ou le ſegment ANM$=0,76562$: on trouve AE$=0,27994$, AP$=1,43098$, et EP$=1,15104$. On a donc la formule $\frac{2Rr \times 1,15104}{d} = 6' \, 24''$: d'où l'on tire $r = 5' \, 20''$, et lediametre entier, en degrés de 22′ 22″. Ces deux reſultats s'accordent merveilleuſement bien,

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ſo perfectly well with each other, that, if I am not too fond of my own work, I may venture to ſay, the method, I have invented, may be carried to great exactneſs. I hope you will have the goodneſs to give me your opinion of it, which I have the greateſt reſpect for, and will be very uſeful to me, eſpecially if you have leiſure to repeat the obſervations. I ſhall take the liberty to ſubjoin a few hints, upon the manner of making them, at the end of this letter. The diameter of the firſt ſatellite I have determined by three obſervations as follows;

		1771,		′	″		°	′	″	
An	Immerſion	June 30,	gave	7	17	in time or	1	1	45	as ſeen from ♃.
	Emerſion	Aug. 1,		7	3		0	59	46	
	Emerſion	Sept. 2,		7	1		0	59	29	

You ſee, Sir, that this agreement is likewiſe very ſatisfactory. Mr. Meſſier took part in theſe obſervations, and found the application of them very eaſy. He himſelf

bien, et ſi je ne m'abuſe pas trop ſur mon travail, il me ſemble, que la méthode, que je viens de vous expoſer, eſt ſuſceptible d'une très grande exactitude. J'eſpere que vous voudrez bien m'en dire votre ſentiment, dont je fais le plus grand cas, et qui me ſera très utile, ſurtout ſi vous avez le loiſir de répéter ces obſervations. Je prendrai la liberté d'ajouter, à la fin de cette lettre, quelques réflexions ſur la maniere de les faire. A l'égard du diametre du premier, je l'ai déterminé par trois obſervations. L'immerſion du 30 Juin 1771, m'a donné ſon diametre de 7′ 17″ en tems, ou de 1° 1′ 45″ vû de Jupiter. Le premier Août l'émerſion m'a donné ſon diametre de 7′ 3″ en tems, ou de 0° 59′ 46″; enfin, l'émerſion du deux Septembre m'a donné ſon diametre de 7′ 1″ en tems, ou de 0° 59′ 29″. Vous voïez, Monſieur, que cet accord eſt encore très ſatisfaiſant. Mr. Meſſier a bien voulu prendre part à ces obſervations, il en a trouvé l'uſage facile.

Il

ſelf obſerved the diameter of the ſecond ſatellite, by the emerſion of the 30th of Auguſt; but it emerged at ſo ſmall a diſtance from the firſt, that this circumſtance may have vitiated the obſervation; the diameter of this ſatellite muſt therefore be verifyed by freſh obſervations. However, the reſult of Mr. Meſſier's makes it 7′ 2″ in time, or 29′ 42″ ſeen from Jupiter; and a former obſervation of mine, of the 11th of July, gave the ſame quantity preciſely. Thus, by means of the tables given in my paper, which I had the honour of mentioning to you, it will be poſſible, to compute the inviſible ſegment, for all the obſervations which have been hitherto made, and, the diameter of the ſatellite being likewiſe aſcertained, to reduce the inſtant of the obſerved eclipſe to that of the paſſage of the center over the edge of the ſhadow, which will be a fixed term for all the obſervations, and

Il a obſervé lui même le diametre du ſecond, par l'émerſion du 30 Août, mais il ſortit ſi près du premier, que cette circonſtance peut avoir nui à la bonté de l'obſervation; ainſi le diametre de ce ſatellite a beſoin d'être confirmé par de nouvelles obſervations. Quoiqu'il en ſoit, il réſulte de ſon obſervation, que le diametre du ſecond eſt de 7′ 2″ en tems, ou de 29′ 42″ vû de Jupiter. Je l'avais trouvé par une autre obſervation du 11 Juillet, de 7′ 2″, préciſement comme Mr. Meſſier. Au moïen des tables, que j'ai données dans mon mémoire, et dont j'ai eu l'honneur de vous parler, Monſieur, on pourra donc calculer, pour toutes les obſervations faites juſqu'à préſent, le ſegment inviſible, qui a eu lieu, et, au moïen du diametre connu réduire l'inſtant de l'éclipſe, au moment où le centre s'eſt trouvé ſur le bord de l'ombre, moment qui ſera un terme fixe pour toutes les obſervations, et pour tous les obſervateurs,

and all the obſervers, who but ſeldom agree in the obſervation of the ſame eclipſe. I confeſs, that the tranſparency of the air is not always the ſame, and that a greater or leſs degree of tranſparency will make the ſegments ſmaller or larger, and conſequently affect the obſervation. The inequality of ſight may likewiſe occaſion ſome error; for, though it might be poſſible, to ſettle the general effect of the difference of ſight of different obſervers, the ſight of the ſame perſon is not conſtantly the ſame, and even independently of the change produced by age, may not have the ſame ſtrength at all times. But by the method I propoſe, all theſe inconveniencies will be remedied, in future obſervations, with little trouble. Every obſerver is to furniſh himſelf with ſeveral diaphragms of paſteboard, gradually diminiſhing by half-lines, to be applied to the object-glaſs externally, and ſome minutes before an immerſion, or after an emerſion, he is to determine which of them intercepts

teurs, qui ne s'accordent que rarement dans l'obſervation de la même éclipſe. Je conviens, que la tranſparence de l'air n'eſt pas toujours la même, et que quelques degrés de plus ou de moins dans cette tranſparence rendent les ſegmens moins ou plus grands, et conſéquemment affectent l'obſervation. L'inégalité des vuës eſt encore une autre ſource d'erreurs; quand même on pourrait établir l'effet de la difference des vuës des obſervateurs; ces vuës ne ſont pas conſtantes, et, ſans parler du changement, que l'age amene avec lui, la vuë peut n'avoir pas la même force dans tous les inſtans. Voici la méthode, que je propoſe, pour remedier à tous ces inconveniens, d'une maniere facile et commode, à l'égard des obſervations futures. Il ſuffirait, que chaque obſervateur fit des diaphragmes de carton, pour appliquer exterieurement à ſon objectif, qui diminuaſſent de demi ligne en demi ligne. Quelques minutes avant une immerſion, ou après une émerſion, il

intercepts from him the sight of the satellite. Having found this, and knowing likewise the diameter of the satellite, he will reduce [by the process of calculation already explained] the observed instant of the eclipse, to that of the passage of the center; which is the same [as I said before] for all the observers in the world. You see, Sir, what advantages would arise, from this agreement, for the theory of the satellites, and the precision of the terrestrial longitudes. This method takes in every thing; the difference of glasses, that of sights, the greater or less transparency of the atmosphere, &c. Observation gives the segment greater or less, in proportion to the combined influence of all these causes. The principal advantage of this method, which requires only a very simple calculation, is, that it depends on no hypothesis. It enables us to measure immediately the light of the satellite, whether increased or diminished by all the causes above-mentioned; to measure, I say, the real impression of that

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il déterminerait, lequel de ces diaphragmes lui dérobe la vuë du satellite: ensuite, au moyen du diametre, il reduirait l'instant de l'éclipse observée, au moment du passage du centre, moment qui est le même pour tous les observateurs de tous les pays du monde. Vous voyez, Monsieur, quels avantages il résulterait de cet accord, pour la theorie des satellites, et pour la précision des longitudes terrestres. Cette méthode renferme tout; la différence des lunettes, celle des vuës, le plus ou moins de transparence de l'atmosphere, &c. C'est en raison de toutes ces causes, que l'on trouve, par observation, le segment plus ou moins grand. L'avantage principal de cette méthode, qui n'exige qu'un calcul fort simple, est, de n'être soumise à aucune hipothese. On mesure immédiatement la lumiere du satellite, augmentée ou diminuee par

that light upon the eye, whatever be the actual ſtate of the organ. Theſe, Sir, are the methods and enquiries which I ſubmit to your judgment, and with that view, I have been very copious in my details. I am highly ambitious of your approbation, and ſhall pay great attention to your remarks. I muſt add, that I am ſenſible, the determination of the inviſible ſegment, by means of the diaphragm, might be inconvenient to thoſe, who make uſe of large teleſcopes for the eclipſes of the ſatellites, were it not, that this obſervation may be equally well made with a ſmaller teleſcope, provided only, that it be ſufficicent to ſee and diſtinguiſh the four ſatellites; and after the diaphragm is determined by this ſmaller teleſcope, the larger one may be uſed for the obſervation of the eclipſe. For theſe meaſures are eaſily transferred from one inſtrument to another, the inviſible ſegments in different teleſcopes, being inverſely as

par toutes les cauſes que nous avons indiquées, et on meſure même l'impreſſion de la lumiere ſur l'œil, quel que ſoit l'état actuel de cet organe. Voila, Monſieur, les méthodes et les recherches, que je ſoumets à vos lumieres; et je l'ai fait avec beaucoup de détail, afin de vous mettre dans le cas de les juger. J'ambitionne extremement votre ſuffrage, et je ferai uſage de vos reflexions. J'ajouterai, que la détermination du ſegment inviſible, par le moyen du diaphragme, pourrait devenir incommode, pour ceux qui ſe ſervent de grandes lunettes, dans les éclipſes des ſatellites; mais cette obſervation peut ſe faire également avec une petite lunette, pourvû qu'elle faſſe bien voir et diſtinguer les quatre ſatellites. Quand on aura déterminé le diaphragme avec cette lunettte, on fera l'obſervation de l'éclipſe avec ſon inſtrument ordinaire. On peut tranſporter ces meſures, très aiſément, d'un inſtrument à l'autre, puiſque les ſegmens

as the ſquares of the apertures. For reflectors, I have a method of the ſame kind with the former, grounded, at leaſt, upon the ſame principles, by which I can determine their power, and compare them, both with each other, and with the refracting teleſcopes. I ſhall conclude with ſome hints concerning the obſervation of the diaphragm, for determining the inviſible ſegment. To repeat theſe obſervations with judgement, it will be neceſſary to recollect the intention of them; which is, to meaſure what portion of the diſc remains illumined, that is, what portion of the ſatellite's light [continues, though unperceived, to be tranſmitted to the obſervers eye] at the inſtant when the ſatellite diſappears, upon the brink of an eclipſe. In leſſening gradually the aperture of the glaſs, the obſerver ſhould not begin with too ſmall an opening; becauſe the eye, not accuſtomed

to

mens inviſibles ſont, dans les differentes lunettes, en raiſon inverſe du quarré des ouvertures. Quant aux téleſcopes de reflexion, j'ai une méthode à peu près du même genre que celle-ci, et qui du moins eſt fondée ſur le même principe, par laquelle je puis déterminer la force des teleſcopes, et les comparer, tant entr'eux, qu'avec les lunettes. Je finirai, Monſieur, par quelques réflexions ſur l'obſervation du diaphragme, qui détermine le ſegment inviſible. Pour bien répéter ces obſervations, il faut ſe rapeller l'eſprit, dans lequel elles ont été entrepriſes, et dans lequel elles doivent toujours être faites. C'eſt de meſurer la quantité de lumiere, la portion éclairée du diſque, qui ſubſiſte encore au moment où le ſatellite diſparait, en s'éclipſant. Quand on diminuë, par degrés, l'ouverture de la lunette, il ne faut pas commencer par une trop petite ouverture: l'œil n'eſt pas fait à la grande obſcurité qui en réſulte; on ne verrait point le ſatellite. A chaque

fois

to the great obſcurity which follows, might not ſee the ſatellite at all. As the opening is gradually contracted, the ſatellite ſeems to grow leſs. The obſerver ſometimes loſes ſight of it for a moment; but, if he continues to look attentively, he ſees it again. The real diſappearance is only to be concluded, when, upon fixing with ſteady eyes, for about half a minute, on the place it occupied, it is ſeen no more; for if one perſiſted to obſerve it much longer, it might happen, that it might be ſeen to glimmer at times, and immediately diſappear. I have always made it a rule, to conſider the debilitation of the light, in this degree, as actual diſparition, and it is neceſſary, that obſervers ſhould agree upon this point, in order that their different eſtimations may be conſiſtent. Theſe fits, of momentary glimmering and extinction, are undoubtedly owing to the motion of the particles of the atmoſphere. In the cleareſt weather, there are always particles of vapour floating in it, in vaſt abundance; according as theſe particles place them-

ſelves

fois qu'on diminuë l'ouverture, on voit diminuer le ſatellite: quelquefois on croit ne le plus voir, mais, après y avoir fait attention, on le revoit: il faut le juger diſparu, quand on ne l'apperçoit plus, après avoir conſidéré, avec des yeux bien repoſés, pendant une demie minute environ, la place qu'il occupe: car, ſi on s'attachait à le conſidérer plus longtems, il pourrait arriver, qu'on le revit, par moment, paraitre, comme par éclairs, & diſparaitre auſſitôt. Je l'ai toujours jugé diſparu, quand il a été réduit à cet état d'affaibliſſement; il eſt eſſentiel de convenir de ceci, afin que les meſures ſe rapportent. Ces variations ſont duës, ſans doute, au mouvement de l'atmoſphere. Il y a toujours, dans les

moments

ſelves in the direction of the ray of light, or out of it, the light of the ſatellite is diminiſhed or reſtored, and the ſatellite, in conſequence, is either hid or rendered viſible. This does not happen in eclipſes, wherein a great part of the light is in reality extinguiſhed. But, in the caſe I am now ſpeaking of, though the diminution of the aperture of the glaſs does indeed take away a great quantity of it, yet this quantity is always relative to the actual ſtate of the atmoſphere ([d]): if that ſtate changes, this quantity becomes alternately ſenſible or inſenſible, according as the light meets with more or leſs obſtructions, in its paſſage, from the vapours. Another thing, which it will be neceſſary to point out to you, is, that the operation with the diaphragms, for determining the inviſible ſegment, muſt be made and concluded, before the ſatellite has touched the ſhadow. The proper time, therefore

momens où le tems eſt le plus ſerein, une infinité de vapeurs, dont les particules nagent dans l'atmoſphere; ſelon que ces particules s'accumulent ſur la route du raïon de lumiere, ou qu'elles s'en écartent, elles ôtent ou rendent plus de lumiere à ce ſatellite, qui, en conſéquence, ſe cache ou ſe laiſſe voir. C'eſt ce qui n'arrive point dans les éclipſes, où une grande partie de la lumiere eſt réellement détruite. Ici l'ouverture diminuée de la lunette en détruit réellement auſſi une grande quantité, mais cette quantité eſt toujours rèlative à l'etat actuel de l'atmoſphere. Si l'état de l'atmoſphere change, cette quantité devient alternativement ſenſible ou inſenſible, ſelon que les vapeurs lui oppoſent plus ou moins d'obſtacles. Une choſe qu'il eſt encore néceſſaire de vous faire obſerver, Monſieur, c'eſt, que l'operation du diaphragme, qui détermine le ſegment inviſible, doit être faite et finie, avant que le

fore, for beginning this observation, will be determined, by the time the diameter takes in entering, which, in the perpendicular ingress, or when Jupiter is in the nodes, is 7′ for the two first, and 11′ for the third. The time of the oblique ingress is $(7') \frac{2R}{d}$ for the two first, and $(11') \frac{2R}{d}$ for the third; which, for this last, may in extreme cases amount to about 27′ or 28′. It is proper to take 5′ more; for the observations of the diaphragm will take up 2′, even when use has rendered them familiar, and the tables may be 2′ or 3′ behind. At present, it is be sufficient, to begin the observation 16′ before an eclipse of the third satellite; but there are times, in which it would be necessary, to begin 29′ or 30′ before. It is essential, not to begin too late, for fear of missing the observation; it is likewise essential, not to begin too soon, because then the segment

le satellite ait touché l'ombre. Ainsi le tems, que le diametre met à entrer, doit donc regler cette observation; c'est à dire, 7′ pour les deux premiers, et 11′ pour le troisième; mais ce tems est celui de l'entrée perpendiculaire, qui n'a lieu que lorsque Jupiter est dans les nœuds. L'entrée oblique est $7' \frac{(2R)}{d}$ pour les deux premiers, et $(11') \frac{2R}{d}$ pour le troisième, ce qui peut aller pour celui ci jusqu'a 27′ ou 28′. Il faut encore prendre 5′ de plus, parceque même étant exercé, il faut bien 2′ pour l'observation du diaphragme, et que les tables peuvent être en retard de 2′ à 3′. Actuellement il suffit donc, de s'y prendre, pour cette observation 16′ avant une eclipse du troisième, mais il y a des tems, où il faudroit s'y prendre 29′ ou 30′ avant. Il est

ſegment meaſured would be too ſmall, as the ſatellite is continually either approaching Jupiter, or receding from him. All this is haſtily explained; but theſe matters are ſo familiar to you, that you cannot but underſtand me, and this letter is already too long. I am afraid it will tire you; but I am extremely deſirous of having the exactneſs and utility of theſe two methods, the one, for the meaſure of the diameters, the other, for making all the obſervations capable of mutual compariſon, aſcertained, by repeating the obſervation of the diaphragm in every eclipſe. I cannot take a better way, than to conſult the ſeveral aſtronomers, who, like you, beſides being deeply ſkilled in the theory, are the moſt celebrated obſervers. If they will adopt this method, it will be the beſt way of making it general, as others will follow it of courſe. I have communicated

eſt eſſentiel de ne s'y pas prendre trop tard, pour ne pas manquer l'obſervation; il eſt eſſentiel auſſi de ne s'y pas, prendre trop tôt, parcequ' alors on meſurerait un ſegment trop petit; car le ſatellite s'approche ou s'éloigne continuellement de Jupiter. Tout ceci eſt expliqué en courant, comme vous voyez, Monſieur, mais vous en ſavez trop ſur ces matieres, pour ne me pas entendre, et cette lettre eſt déja d'une exceſſive longueur. J'ai bien peur, qu'elle ne vous cauſe de l'ennui; mais j'ai extremement à cœur, de conſtater l'exactitude et l'utilité de ces deux méthodes, celle de la meſure des diametres, et celle de rendre toutes les obſervations comparables, en répétant, à chaque éclipſe, l'obſervation du diaphragme. Je ne puis mieux faire que de conſulter les aſtronomes, qui étant comme vous, Monſieur, profondement verſés dans la theorie, ſont, en même tems, les plus célébres obſervateurs. Si ceux ci veulent bien adopter cette méthode, c'eſt le meilleur moyen de la rendre générale: les autres l'adopteront auſſi.

4 Je

municated it to Mr. Messier, who proposes making use of it. Mr. Maraldi, who is gone to his house near Nice, has tried the observation of the diaphragm, with success, with an achromatic telescope three feet and a half long; and he would already have made use of it, but that it is impracticable with the telescope of 15 feet, which he uses for the eclipses of the satellites. I have written to him, that he may observe the eclipse with his usual telescope, and the diaphragm with the achromatic; so that I make do doubt he will use this method, as soon as Jupiter shall have come out of the rays of the Sun. These, Sir, are the things on which I wish to consult you, and have your advice. I shall be much

Je l'ai communiquée à Mr. Messier, qui se propose d'en faire usage. Mr. Maraldi, qui, comme vous savez, est allé chez lui à Perinaldo, dans le comte de Nice, a essaié l'observation du diaphragme, qui lui a réüssi, avec une lunette achromatique de 3 pieds et demi; et il l'auroit déja emploïée, si ce n'est, que l'observation du diaphragme n'est pas practicable avec sa lunette de 15 pieds, dont il se sert pour les éclipses des satellites. Je lui ai mandé, comme je vous l'ai fait observer plus haut, qu'il pouvoit faire l'observation du diaphragme avec sa lunette achromatique, et celle de l'éclipse avec sa lunette ordinaire: ainsi je ne doute point, qu'il ne fasse usage de cette méthode, lorsque Jupiter se dégagera des raïons du soleil. Voila, Monsieur, toutes les choses, sur lesquelles je suis bien aise de vous consulter, et de vous demander votre avis. Si vous croyez que cette lettre en vaille la peine, je serai très flatté, que vous la communiquiez à la Societé Roïale, à laquelle je présente l'hommage de mon profond respect. Pardon,

much flattered by your communicating this letter to the Royal Society, if you think it deserves attention.

I have the honour to be, &c.

Bailly,

Of the Royal Academy of Sciences of Paris.

Pardon, Monsieur, de vous avoir détourné si longtems. Permettez moi de vous assurer du respect avec lequel je suis,

Votre très humble

Et très obeïssant Serviteur,

BAILLY,

De l'Academie des Sciences de Paris.

NOTES

NOTES ON THE FOREGOING PAPER,

BY THE REV. SAMUEL HORSLEY.

(a) $\frac{2\,R\,r\,(a-b)}{d}$.

This formula is deduced from the following principles.

1ft. That the motion of the fatellite, in its orbit, is uniform; or, at leaft, may be confidered as fuch, without fenfible error, in the prefent inveftigation.

2. That the time, which the femidiameter takes to enter the fhadow, in any eclipfe, is inverfely as the whole time of the duration of the eclipfe.

3. That the time, which any given part of the femidiameter takes to enter the fhadow, is to the time which the whole femidiameter takes to enter, as that part to the whole.

Now, let a and b denote the verfed fines of the arcs Ad, AD (in the figure p. 189.) refpectively, the radius being unity. Let R denote the half-time of the duration of an eclipfe, when Jupiter is in the node of the fatellite's orbit. r, the time which the femidiameter takes to enter the fhadow in *fuch* eclipfes; d, the whole duration of an eclipfe, happening when Jupiter is at any given diftance from the node. Then will $\frac{2\,R\,r}{d}$ exprefs the time, which the femidiameter of the fatellite will take to enter the fhadow, in the eclipfe whofe duration is d (by 2^{a}, becaufe $d : 2\,R = r : \frac{2\,R\,r}{d}$). And, $\frac{2\,R\,r}{d}$ being the time that the femidiameter takes to enter the fhadow, $\frac{2\,R\,r\,(a-b)}{d}$ will be the time that the part Bb takes to enter, by 3^{d}."

It is to be obferved, that, to compare two eclipfes by this formula, it is neceffary, that the planet fhould have been at the

ſame diſtance from the node of the ſatellite's orbit, at the commencement of both. For comparing eclipſes otherwiſe circumſtanced, a more general formula may eaſily be deduced from the ſame principles. If Ad be the inſenſible ſegment in one eclipſe, AD in another (vide figure p. 189.), a the verſed ſine of the arc Ad, b of AD (the radius being unity), d the whole duration of the firſt eclipſe, δ of the ſecond, then $\frac{2Rr}{d\delta} \times \overline{\delta a - db}$ is what the author would call the equation, between the two, ariſing from the different magnitudes of the inſenſible ſegment, or the time, by which the interval between the obſerved eclipſes, differs from the interval between the real paſſage of the center in each eclipſe. This is a general formula, for all eclipſes of the ſame ſatellite. If the planet's diſtance from the node hath been the ſame in both, then $\delta = d$, and this formula changes into the author's. The more general one is here given, rather for the fuller explication of the theory, than for any neceſſity that there is to have recourſe to it in practice. For, though the uſe of it may ſometimes be convenient, eclipſes of the ſame ſatellite may always be compared without it, when once the diameter of the ſatellite is known, and the magnitude of the inſenſible ſegment in each eclipſe determined, by reducing the obſerved immerſion or emerſion to the true ingreſs or egreſs of the center.

(b) The words printed in the Italic character are deſignedly omitted in the tranſlation, it being apprehended, that it is owing to ſome inadvertency, that they appear in the author's text. For unleſs they are expunged, the general deſcription, here intended, of the author's method of determining the diameters of the ſatellites, will by no means agree with the examples of that method immediately ſubjoined. Theſe words imply, that the author takes the inſtant of the diſparition of the ſatellite, in the contracted aperture of the diaphragm, for the moment of the contact of the ſatellite's limb with the edge of the ſhadow, and makes that moment, ſo determined, the baſis of his calculations . reaſoning, as it ſhould ſeem, thus. " When any part of the diameter of the ſatellite, however ſmall, hath entered the ſhadow, ſome part of the light, which the obſerver receives, through the aperture of the diaphragm, from the whole unſhaded diſc, will be intercepted. But that aperture is ſo ſmall, that the light tranſmitted through it, from the whole unſhaded diſc, is but juſt ſufficient to be ſenſible; and muſt, therefore, ceaſe to be ſo,

when it is in the smallest degree diminished, i. e. when the very smallest part imaginable of the disc is shaded. Therefore, the moment of the disparition, in the aperture of the diaphragm, is the true commencement of the eclipse, or differs from it by less than any assignable difference."

But it appears, from the examples given afterwards, that the author's calculations proceed upon much safer principles. Having determined the portion of the disc, that is insensible on the whole given aperture of his telescope, he computes what larger portion will be insensible, on the smaller given aperture of his diaphragm. And then, by observing the two disparitions, the earlier one in the diaphragm, the other in the telescope with the object-glass uncovered, the last of which he calls the true immersion, he knows the time, in which a given portion of the diameter enters the shadow, and, consequently, the time, in which the whole enters; which determines the magnitude of the whole, in parts of the satellite's orbit, or its apparent magnitude to an observer at Jupiter's center.

(c) The disadvantage of using too great an aperture is, that the part of the diameter obtained by observation, from which the whole is to be concluded, will be less, than the same method of observation would give, with a more contracted aperture. For the larger the aperture of the diaphragm is, which is applied to the object glass, the less is the difference between that aperture and the whole aperture of the telescope; consequently, the less is the difference between the segments, which are insensible in these apertures severally, and the less the portion of the diameter, which passes over the shadow's edge, between the two disparitions.

(d) In eclipses, when once the satellite hath disappeared, or is become visible, the author says, we are not to expect those fits of glimmering and extinction, which he hath described as taking place, when we observe the uneclipsed satellite with very contracted apertures.

The reason is plainly this. In immersions, a part of the disc is still indeed enlightened, when the satellite disappears; and the quantity of light, transmitted from this part to the observer's eye, must be very different, in different states of the air's transparency; and consequently, the satellite, after having disappeared, might become visible again, by a sudden increase of the air's transparency in the tract of the satellite's light, provided, the

the magnitude of the unſhaded part remained, at the inſtant of the increaſed tranſparency, what it was, when the ſatellite firſt diſappeared. But, as this is not the caſe, as the unſhaded part is continually growing leſs, the ſatellite cannot re-appear, unleſs the increaſe of tranſparency be ſuch, as to overbalance the diminution of light made by the progreſs of the eclipſe. And the motion into the ſhade is ſo quick, that this can rarely, if ever, happen. By the like reaſoning, fits of extinction are not to be expected, when once the ſatellite hath ſhewn itſelf in an emerſion.

The author of theſe remarks doth not imagine, that any apology is neceſſary for the liberty he hath taken. He hath the higheſt opinion of the merit of Mr. Bailly's invention; and this hath excited him, to contribute what he could, to obviate objections, and to prevent miſtakes.

XXVI. *A Letter from Mr.* Bernard, *of* Deptford, *to Mr.* Robertſon, *Lib. R. S.* *containing a ſhort Account of an Exploſion of Air, in a Coal-Pit, at* Middleton, *near* Leeds *in* Yorkſhire.

S I R,

Read Feb. 25, 1773.

I Have at length procured, from my father, a memorandum made by him on the ſpot, of the effects of foul air ſet on fire, which I have copied exactly. If it affords you the leaſt ſatisfaction, I ſhall be well pleaſed, though late, in having fulfilled my promiſe.

I am, with great reſpect,

Yours,

W. Barnard.

Grove-Street, Deptford,
January 12, 1773.

MEMO-

MEMORANDUM.

BEING engaged in Middleton wood, the eſtate of —— Brandling, Eſq; near Leeds in Yorkſhire, in directing the falling and barking of a large quantity of timber bought of him in May 1758, I was witneſs of the following accident. Some miners, being to renew their operations on the ſhaft of a coal-pit, which, in a former year, had been ſunk to the depth of ſixty yards, in order to get through a ſtratum of very hard ſtone, thought proper to drill holes, and fill them with gunpowder. They afterwards, from the top, threw down fire to blaſt the ſtone, which made a report little louder than that of a piſtol; but the blaze, ſetting the foul air on fire, produced an effect truly ſhocking. The whole wood was ſhaken, the works at the mouth of the pit were all blown to pieces, and the exploſion was ſuch as cannot be deſcribed. The vacuum in the air was ſo conſiderable, that oak trees of a load or more each, at a great diſtance from the pit's mouth, that before ſtood upright, ſtooped towards the pit very much, and muſt have fallen wholly down, had not the air been inſtantly replaced. The bark-pullers, at a quarter of a mile from the pit, were ſo alarmed by the ſhaking and exploſion, that not one of them would have remained in the wood, had they attempted to blaſt it again.

N. B. The trees in the whole circuit ſtooped towards the pit.

THE END OF VOLUME LXIII. PART I.

PHILOSOPHICAL

TRANSACTIONS.

VOL. LXIII. PART II.

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Preſent Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Conſiderable Parts of the WORLD.

VOL. LXIII. PART II.

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M.DCC.LXXIV.

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VOL. LXIII. PART II.

XXXII. *An*

XXXIX. *Of*

XXVII. *Extract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon *in* Rutland, 1772, *by* T. Barker, *Esq; Communicated by Sir* John Pringle, *Bart. P. R. S.*

Read March 4, 1773.

		Barometer.			Thermometer.						Rain.
					In the houſe.			Abroad.			
		Higheſt.	Loweſt.	Mean.	High.	Low.	Mean.	High	Low.	Mean	
Jan.	Morn.	29.94	28.35	29.26	44	31	36	45	10	30	2.145
	Aftern.				45	30½	37	51	25	35½	
Feb.	Morn.	29.65	28.65	29.11	46	29	37	46	13	31	3.477
	Aftern.				45½	30½	38	52	24	38½	
Mar.	Morn.	29.64	28.61	29.20	47½	33	41	47	23	34	2.346
	Aftern.				49	34½	42	57	32	43½	
April	Morn.	29.90	28 [illegible]	29.50	51	37½	44½	50	27	39	0.882
	Aftern.				52	40	46	57	35	48	
May	Morn.	30.02	29.07	29.66	55	45½	50½	55	33½	45	1.869
	Aftern.				56	46	52	68	47	55½	
June	Morn.	29.99	29.07	29.66	68	51½	59½	66	47	56	3.890
	Aftern.				70	53½	61	81	58½	68	
July	Morn.	29.99	28.96	29.61	65	59½	62	64	48½	56	0.891
	Aftern.				66	60½	63	73	62	68	
Aug	Morn.	29.91	28.95	29.51	67	56½	61½	61	44	54½	1.678
	Aftern.				68	60	63	76	61	67½	
Sept	Morn.	29.84	28.57	29.42	62½	52½	58	61½	40	51	4.515
	Aftern.				65	54	59	71	54½	60½	
Oct.	Morn.	29.95	28.77	29.52	58	51½	55	57	39	48	3.267
	Aftern.				59	52	56	64½	47½	57½	
Nov	Morn.	29 93	28.50	29.26	55½	41	47	56	31½	41	2.461
	Aftern.				56	42	48	59	39½	46½	
Dec.	Morn.	30.06	28.78	29.57	48½	36	42½	50	24	38	1.226
	Aftern.				49½	36½	43	51½	28½	40½	
											28.647

January began mild, but ſoon inclined to froſt; and about the middle of the month, a ſevere ſeaſon ſet in; much froſt, and great ſnows, which would have been very great indeed, if it had been all lying together; but they were intermixed with thaw, rain, and floods, and continued to the middle of March. This cauſed a great expence of hay, and, with the very backward ſpring, and frequent cold weather, even to the middle of May, made few people have any conſiderable quantity of hay left. There were, however, ſome intervals of mild weather, and more graſs this ſpring than laſt.

The ſummer was, in all the ſouth of England, very dry, and burning. There was ſo little graſs about London, that many were forced to fodder their cattle, even in the height of it; but in all the middle of England, the ſummer was a very fine one; no cold weather, nor in general very hot, but chiefly very fair and fine, and a ſufficient quantity of rain came, whenever we wanted it: ſo great a quantity of hay, ſo well got, was hardly ever known. The beginning of harveſt was alſo well got in, but the latter part of it, for it was a late one, was, in this country and northward, exceeding bad; and in this wet weather, ſome wheat ſuffered, moſt of the barley, and all the beans and peaſe. The crop of wheat, where it was well gotten, was tolerable good, the barley yielded worſe, white peaſe were plentiful enough, but ill got, beans and grey peaſe univerſally a failing crop, much of the ſeed being burſt in the wet ſeed-time. This bad latter part of harveſt was in September, which was all very wet, as it was alſo, in a leſs degree, till the beginning of De-

cember,

cember, but ſo warm, that the graſs grew till the middle of October as faſt as in ſummer; but the wheat ſeed-time was bad, which did but indifferently on wet land, though it came up very well on light ſoils. The ſeaſon continued mild and open, the graſs ſpringing, and ſcarce a morning that could be called froſty, till December 22, when a calm and moderate froſt concluded the year.

Wet ſummers make plenty of graſs, but drier ſeaſons are more favourable to corn. The moſt plentiful ten years in my time were from 1741 to 1750, which were alſo the drieſt; and the moſt failing ſeries of crops have been ſince that remarkable wet year 1763; ſince which, there has been, in general, much more rain than before.

P. S. Be pleaſed to correct the following errata, in my former letter, Phil. Tranſ. Vol. LXI. p. 223.

	For	Read
Feb. 10.	E. by W.	E. by N.
16.	E. by W.	E. by N.
Aug. mean 36 to 70.	2.194.	2.184.
Nov. 1737.	9.570.	0.570.

XXVIII. Ob-

XXVIII. *Obſervations on the Lagopus, or Ptarmigan; in a Letter from the Hon.* Daines Barrington, *V. P. R. S. to* Mathew Maty, *M. D. F. R. S.*

DEAR SIR,

THE many different ſpecimens of Lagopi, both in their winter and ſummer plumage, which have lately been preſented to the Royal Society, from Hudſon's Bay, enable us to correct many miſtakes that have hitherto been made in the deſcription of this bird; as well as the unneceſſarily multiplying the ſpecies of the Tetrao genus.

As that able and ingenious naturaliſt, M. de Buffon, is the laſt ornithologiſt who hath made any obſervations on this bird, it may not be improper to take notice of ſome of his ſuppoſed inaccuracies.

The Lagopus, of which M. de Buffon gives an engraving, is in its winter plumage; and the feet of the bird are conſequently covered very thick with feathers. M. de Buffon, however, from not having examined the ſpecimens of the Lagopus with proper attention, ſays, that Ariſtotle could not have been acquainted with this bird, becauſe the under parts of the claws are entirely covered with feathers; which circumſtance is ſo very ſtriking and peculiar, that it could not have eſcaped this father of natural hiſtory.

If a winter ſpecimen, however, of the Lagopus, or Ptarmigan, is accurately examined, it will be found,

found, that no feathers grow preciſely under the claws; though, by wrapping very thickly round them, they have very ſtrongly that appearance: and, in a ſummer ſpecimen, not only the feet, but even the legs, are rather bare of plumage. If Ariſtotle, therefore, had procured the bird in its ſummer dreſs, he could not have obſerved this very ſtriking circumſtance, which M. de Buffon relies upon as ſo ſtrongly characteriſtic.

The ſame difference between the plumage in ſummer and winter is experienced in each of the three ſpecies of Tetrao, which have (according to one of Linnæus's ſubdiviſions) feathered feet; and it is uſually ſaid with us, that they have in winter their ſnow-boots. M. de Buffon, therefore, unjuſtly charges the author of the Britiſh Zoology for ſuppoſing, that this is a wiſe proviſion of Nature againſt the inclemency of the ſeaſon, when he ſays, [a] that the vrogallus Minor, or our Black Cock, hath not the ſame protection for its feet, though it buries itſelf under the ſnow, and, becoming torpid, equally wants ſuch additional warmth.

With regard to the torpidity of this bird, M. de Buffon relies upon Linnæus's aſſerting, that *ſæpe ſepelitur in nive* [b]; which by no means ſignifies that the bird is torpid, but only that it buries itſelf, ſometimes, under the ſnow; as ſheep do with us in the more rigorous ſeaſons, when it lies very deep in the mountains.

[a] T. ii. p. 216.
[b] Linn. S. Nat. p. 159. This circumſtance is alſo obſerved by Pontoppidan, Pt. ii, p. 75. Engl. Tranſl.

THE

The Black Cock, however, is ſo far from being torpid in the winter, that it even approaches the habitation of man when diſtreſſed for food; and I ſhall likewiſe conclude, till I ſee a ſpecimen which proves the contrary, that, like the other Tetraos, whoſe feet are covered low with feathers, this part of the plumage becomes thicker in winter.

M. de Buffon alſo ſeems to be miſtaken in ſuppoſing, that the thick plumage round the feet is peculiar to the Lagopus; as it is believed, that Linnæus's firſt diviſion of this genus have all of them the ſame additional cloathing for the winter; nor is this extraordinary warmth confined merely to this genus, as the noble ſpecimen of the large White Owl, which hath lately been preſented to the Royal Society from Hudſon's Bay, is covered about the claws with a plumage of perhaps an equal thickneſs.

The next remarkable circumſtance in this bird is, that the ſhafts of many of the wing-feathers are black; which M. de Buffon ſuppoſes to be only ſix; whereas they are eight in the ſpecimens from Hudſon's Bay; the two laſt are, indeed, of a fainter colour.

M. de Buffon next ſays [c], that Briſſon counts eighteen feathers in the tail; and Willoughby, ſixteen; which he reduces himſelf to fourteen. It ſeems to me, however, that Willoughby's number is the more accurate; and, by examining the difference between the ſummer and winter ſpecimens, I find that the black feathers of the tail are covered by two upper ones, which in ſummer are brown, and in winter white.

[c] T. ii. p. 264.

I cannot

I cannot alſo diſcover, in any of the ſpecimens, the two white feathers in the tail, according to Linnæus's deſcription, *rectricibus nigris apice albis, intermediis albis,* as the two covering feathers before-mentioned cannot, with propriety, be termed *intermedii*; nor are they white in the ſummer, but brown: ſo that Linnæus makes a circumſtance, which varies with the ſeaſon, to be a permanent characteriſtic of the bird.

M. de Buffon next ſuppoſes [*d*], that Willoughby and Friſch ſpeak of different birds under the name of Lagopus; becauſe the firſt ſays, that the feet are covered with ſoft, and the latter, with harſh and briſtly feathers. The remarks, however, of theſe ornithologiſts, are eaſily reconciled; for, if the finger is drawn according to the courſe of the feathers, they feel ſoft; and, if in the contrary direction, harſh and briſtly. The difference alſo between Belon, Geſner, and Linnæus, with regard to the call of this bird, is as eaſily accounted for; becauſe moſt male birds differ from the female in this reſpect, and ſometimes the young birds from thoſe which are full-grown.

This naturally brings me to ſhew, that M. de Buffon (who hath great merit in other parts of his Natural Hiſtory, by not unneceſſarily multiplying the ſpecies of animals,) hath, in this kind of Tetrao, conſidered as two ſpecies what, when properly examined, will turn out to be only the Lagopus, or Ptarmigan.

His chief reaſon for conſidering the Lagopus of Hudſon's Bay, as being diſtinct from the Ptarmi-

[*d*] T. ii. p. 271.

gan, arises from his asserting, that Mr. Edwards, in his description of that bird, says, that it is twice as large. Mr. Edwards, however, only considers the size of the Hudson's Bay Lagopus as between that of a Pheasant and a Partridge; in which he is very accurate: the bird is not only evidently so to the eye, but weighs three ounces more than a common Partridge [e].

M. de Buffon likewise seems to make an unnecessary species of Tetrao, under the name of *le petit Tetras, à plumage variable*; as his principal argument for this opinion is, that they are not found on the mountains, as the Lagopi are.

Now, it is very clear, from the name given in the catalogue from Hudson's Bay to this bird, of the *Willow Partridge*, that it lives entirely in that part of the world on the plains; nor are there (it is believed) any very high mountains in the neighbourhood of our forts.

When M. de Buffon, therefore, conceives, that the Lagopus is always endeavouring to find out snow and ice, and that it carefully avoids the glare of the sun [f]; it should seem, that the observation is by no means generally true; because, though the rigour of a Hudson's Bay winter is great, yet the summer is very pleasant, and the snow soon disappears, without which M. de Buffon imagines that the bird cannot exist; though his ninth plate represents the Ptarmigan, in his winter dress, sur-

[e] The Partridge, when full-grown, weighs thirteen ounces, and the Ptarmigan, sixteen.

[f] T. ii. p. 272.

rounded

rounded with trees and plants in moſt luxuriant ſoliage and vegetation.

I cannot agree, moreover, with M. de Buffon, when he ſays, that the fleſh of the Lagopus is bitter; as I have eat them myſelf in the Highlands of Scotland; nor ſhould I have been able to diſtinguiſh the taſte from that of the common Grous, which is well known to be a bird of moſt excellent flavour.

What I have hitherto remarked is chiefly in anſwer to that ingenious naturaliſt, M. de Buffon, who is the laſt ornithologiſt that hath both deſcribed and engraved this bird. I have only one new obſervation to make myſelf; which is, that the claws are ſcooped off at the end exactly like a writing-pen (wanting indeed the ſlit); which circumſtance may likewiſe be ſeen in the claws of our common Grous, or Heath-game, though the reſemblance is not quite ſo ſtrong as in the Ptarmigan.

I ſhall now conclude with copying, from the catalogue tranſmitted with the ſpecimens from Hudſon's Bay, what further relates to the Lagopus; which, as I obſerved before, is there called a Willow-partridge [g].

" The Willow-partridges gather together in large
" flocks in the beginning of October, harbouring
" amongſt the willows, the tops of which are their
" principal food; they then change to their winter

[g] It is not at all extraordinary, that it ſhould there be conſidered as a Partridge; becauſe the White Partridge is the name given to this bird by the old ornithologiſts, who have very naturally conſidered edible birds nearly of the ſame ſize, as Partridges when they have ſhort tails, and as Pheaſants, when they have long ones.

 " dreſs.

" dreſs. They change again in March, and have " their complete ſummer dreſs by the latter end of " June. They make their neſt in the ground in " dry ridges; and are ſo plentiful, that ten thou- " ſand have been killed in the three forts in one " winter."

I am, dear SIR,

Your moſt faithful,

humble Servant,

Daines Barrington.

XXIX. *Account of the Effects of Lightning at* Steeple Afhton *and* Holt, *in the County of* Wilts, *on the 20th of* June, 1772, *contained in feveral Letters, communicated by* Edward King, *Efq; F.R.S.*

To Edward King, Efq;

SIR,

Read March 18, 1773.

I Have fent you, agreeably to your requeft, fome account of a ftorm of thunder and lightning that happened at Steeple Afhton, in Wiltfhire, on the 20th of June laft. I was from home when it happened; but the truth of it is attefted by the Reverend Mr. Wainhoufe of Steeple Afhton, and the Reverend Mr. Pitcairn of Trowbridge, who were in my houfe during the tempeft, and were in danger of lofing their lives by it. I have added a defcription of the effects of the fame ftorm upon a houfe at Holt, in this county. Both tend to prove in a remarkable manner the danger of placing any confiderable quantity of iron in the upper part of chimnies, without a conductor to guard againft the ftrokes of lightning. You are at liberty to lay thefe papers before

before the Royal Society, if you think them deserving of their attention.

I am,

SIR,

Your obedient

humble Servant,

Steeple Afhton,
Aug. 10, 1772.

L. ELIOT, Vicar of Steeple Afhton, in Wiltfhire.

ON the 20th of June, 1772, between twelve and one o'clock in the afternoon, a violent ftorm of thunder and lightning happened at Steeple Afhton, in Wiltfhire. During the ftorm, a woman in the village faw a large quantity of lightning come out of a cloud, part of which is fuppofed to have fallen on the top of the north chimney of the vicarage houfe, attracted probably by an iron hoop that went round the chimney, and by fome iron bars placed within it, that formerly made part of an apparatus to prevent its fmoking. That the lightning fell on thefe iron bars is very probable, becaufe the colour of two of them that were contiguous was changed, nine or ten inches in length, to a dark blue, like that of a watch fpring, no uncommon effect of electrical fire.

In the north parlour, to which this chimney belonged, were the Reverend Mr. Wainhoufe, of Steeple Afhton, and the Reverend Mr. Pitcairn, of Trowbridge, the former ftanding, and the latter

ter ſitting in a great chair, with his back to the fire-place, near the wire of a bell. In the ſouth parlour, ſeparated from the other by a hall, were a maid ſervant and a painter; in the kitchen another maid ſervant; in the coal-houſe, four or five yards from the houſe, a man ſervant; near the barn, about fifty or ſixty yards from the houſe, another man ſervant. When the lightning fell upon the houſe, the man ſervant near the barn heard a very loud noiſe, equal, he ſuppoſes, to the ſound of twenty cannons fired at once, and would have fallen to the ground, if he had not caught hold of ſomething to ſupport himſelf. The other man ſervant in the coal houſe was ſtruck backward, and felt ſomething, as he deſcribes it, like a ſtream of warm water poured upon the middle of his body, which, if it was not the electric fluid itſelf, was the heated air expanding itſelf with violence after the exploſion. The maid in the kitchen heard a great noiſe, but received no ſhock. The other maid ſervant, who was ſtanding near *the middle* of the ſouth parlour, ſuffered likewiſe no ſhock, being only terrified exceedingly with the exploſion, and the ſparks of fire, which ſhe ſaw on all ſides of her; but the painter, who was in the ſame room, painting near the chimney and the bell wire, was ſtruck on the left ſide of his body that was next the wire, from his head to his waiſt; he felt in particular a ſevere ſhock, like the electrical one, in his left wriſt, which was marked all round with blue and yellow intermixed; a ſplinter from the wooden caſe, that covered the bell-wire, ſtruck through his glove, and wounded his hand; and he was ſtunned for ſome time.

It

it may be proper to obſerve, that immediately after the woman had ſeen the lightning come from the cloud, as above-mentioned, ſome perſons in the village, beſides thoſe in or near the vicarage houſe, were thrown to the ground.

The following is the account, which Mr. Wainhouſe and Mr. Pitcairn give of what happened in the north parlour in which they were. As they were converſing about a loud clap of thunder that had juſt happened, they ſaw on a ſudden a ball of fire between them, upon a level with the face of the former, and about a foot from it. They deſcribe it to have been of the ſize of a ſixpenny loaf, and ſurrounded with a dark ſmoke; that it burſt with an exceeding loud noiſe, like the firing of many cannons at once; that the room was inſtantly filled with the thickeſt ſmoke; and that they perceived a moſt diſagreeable ſmell, reſembling that of ſulphur, vitriol, and other minerals in fuſion; inſomuch that Mr. Pitcairn thought himſelf in danger of ſuffocation. Mr. Wainhouſe providentially received no hurt, except a ſlight ſcratch in his face from the broken glaſs that was flying about the room, a kind of ſtupefaction for ſome time, and a continued noiſe in his ears, which noiſe, the effect of the exploſion, happened likewiſe to Mr. Pitcairn, and others in the houſe.

The lightning fell on Mr. Pitcairn's right ſhoulder, made a hole in his coat, about a quarter of an inch in diameter, went under his arm in one line to his breaſt, deſcended from thence down the lower parts of his body in two irregular lines, about half an inch broad, attracted probably by

his watch, the glaſs of which it ſhivered into ſmall pieces, and meeting perhaps with a little reſiſtance from it, ſpread itſelf round his body, and produced the ſenſation of a cord, tied cloſe about his waiſt. A violent pain in his loins immediately followed; and from thence to his extremities there ſeemed to be a total ſtoppage of circulation, all ſenſation being loſt, and his legs and feet reſembling in colour and appearance thoſe of a perſon actually dead. Beſides ſhivering the glaſs of his watch, the lightning melted a little of the ſilver of it, and a ſmall part alſo of half a crown in his pocket. When it came to the middle of his thigh, it left an impreſſion of a blackiſh colour, reſembling the branch of a tree, which in a few days diſappeared; but the lines on his body are ſtill viſible, and are of a dark blue, intermixed irregularly with a deep yellow. From the middle of his thigh the lightning changed its direction again, and went down the under ſide of it to the calf of his leg, and ſo to his ſhoe, which *was ſplit into ſeveral pieces* in ſo remarkable a manner, as juſtly to claim the inſpection of the curious. As ſoon as Mr. Pitcairn was ſtruck, he ſunk in his chair, but was not ſtunned; his face was blackened, and the features of it diſtorted. His body was burned in ſeveral places, ſmall holes were made in different parts of his cloaths, and he loſt in ſome meaſure the uſe of his legs for two or three days; but by proper care he ſoon recovered, except a weakneſs and numbneſs in his right leg, which ſtill remains. What is remarkable, Mr. Pitcairn remembers very well to have ſeen the ball of fire in the room for

a ſhort time, a ſecond or two, *after he found himſelf ſtruck with the lightning.* Extraordinary as this circumſtance may appear, it may be proper to take notice, that it is entirely agreeable to an obſervation of the learned and ingenious Dr. Franklin, quoted below *.

The effects of the lightning on the building and furniture were as follows. The north chimney was thrown down, the roof and cieling near it beat in; large ſtones were forced out of the walls, ſome were driven to a conſiderable diſtance, one in particular to about 200 feet. The glaſs of the windows in the north parlour and the chamber over it was forced outwards, except in the caſements, which were open, and in which not a pane of glaſs was broken. The caſe of a clock in the ſame parlour fell forwards, and was beaten to pieces; a looking glaſs over the chimney was thrown on the floor, and broken, ſome of the quickſilver was melted, as was likewiſe ſome of the lead belonging to the windows. A bureau, that was locked, was opened; as was alſo the parlour door, inwards, probably by the external air ruſhing in to reſtore the equilibrium. Some bedding in one of the chambers was fired, but the fire was extinguiſhed of itſelf, or by the rain that fell during the ſtorm, before it was diſcovered. Several ſplinters were torn out of a hogſhead full of

"* In every ſtroke of lightning I am of opinion that the ſtream of the electric fluid, &c. will go conſiderably out of a direct courſe for the ſake of the aſſiſtance of good conductors; and that in this courſe it is actually moving, though ſilently and imperceptibly, *before the exploſion*, in and among the conductors, &c. Franklin's Experiments and Obſervations on Electricity, edit. 4. pag. 124.

beer

beer, but the cask was not materially damaged, nor the beer spilt. The iron bell-wire in both the parlours and the hall was reduced to smoke and entirely dissipated, excepting in those parts where it was twisted, and double, and also the wire springs contiguous to the bell, which the lightning left undamaged, as well as the brass handles and bell itself. The cieling and wall on each side, where the wire went, was stained irregularly, a foot or more in breadth, with a dark blue intermixed with a deep yellow. It is worth observing, that this iron bell wire was very small, considerably less than a common knitting needle; but though it was itself destroyed, yet it seems to have served as a conductor to the lightning, and to have prevented worse effects than happened. For when the lightning had run along, and consumed all the *single* wire, and had reached that which was twisted and double in the south parlour, contiguous to the brass handle, which the bell used to be rung with, it made a hole in the wall of five or six inches in diameter, being attracted probably by an iron stove on the other side in the kitchen chimney, where meeting with several large conductors, andirons, poker, tongs, &c. it seems to have been conveyed into the ground. This appears probable, because the progress of it below stairs could not be traced beyond this hole, which it made in the wall. In the chamber over the kitchen, a small piece of wood was indeed struck out of a bed post, and the glass of half a window was driven outwards; but this does not seem to have been the immediate effect of the lightning, but of the shake from the explosion.

Upon the whole, it muſt be allowed, that Mr. Wainhouſe and Mr. Pitcairn had a moſt wonderful and providential eſcape, particularly the laſt gentleman; for had the lightning paſſed from his ſhoulder in a right line through his body, inſtead of going round upon the ſurface of it, under his arm, in order to come at his watch, by which it ſeems to have been attracted, immediate death would in all probability have been the conſequence.

Whether Steeple Aſhton is from its ſituation particularly expoſed to thunder-ſtorms, is uncertain. It may however be proper to mention, that in the year 1670, July the 25th, a violent ſtorm of thunder and lightning damaged the church ſteeple, which was 93 feet high; and on the 15th of October in the ſame year, another thunder ſtorm threw it entirely down, and killed two of the workmen, who were repairing it.

We have peruſed, and carefully examined the above account, and hereby teſtify the truth of all the facts related in it, to the beſt of our knowledge and belief.

August 22, 1772.

William Wainhouſe.
Robert Pitcairn.

To Edward King, Eſquire.

P. S.

P. S.

I fear the defcription I left at your houfe of the thunder ftorm laft June is too long; you may fhorten it as much as you think proper. I purpofely omitted a few circumftances, which I thought lefs material. Since I was in London, I have been informed of two particulars, which I will beg leave to mention to you.

Mr. Field, a painter of Trowbridge, during the ftorm, obferved a ball of fire vibrate forward and backward in the air over fome part of Steeple Afhton, and at laft dart down perpendicularly, which in all probability was the ball of fire that Mr. Wainhoufe and Mr. Pitcairn faw in the north parlour of the vicarage houfe.

The other circumftance is as follows: After the explofion of the ball of fire in the north parlour, Mr. Pitcairn obferved a great quantity of fire of *different* colours *vibrating* in the room forwards and backwards with a moft extraordinary fwift motion.

The vibration in both thefe cafes obferved by different perfons before and after the explofion is a fact, which I fhould have taken notice of, had I been made acquainted with it fooner.

I am, dear Sir,

Your obliged and affectionate

humble fervant,

Steeple Afhton,
near Trowbridge, Wilts,
Nov. 28, 1772.

L. Eliot.

To

To the Rev. Mr. ELIOT.

SIR,

Hearing of the damage done by lightning to your houſe, on the 20th of June laſt, I have ſent you an account of what happened to mine on the ſame day, and nearly at the ſame time.

During the ſtorm a perſon in this place ſaw a body of fire moving towards a houſe that is next to mine, though at ſome diſtance from it; attracted probably by a large iron bar of ten or twelve feet long, fixed horizontally to ſupport a high chimney. This body of fire changed its direction, and fell on my houſe, forced a brick out of the chimney, near that part of it to which the iron bar was faſtened, and went through the houſe to an outward door on the oppoſite ſide, which happened to be open; there it burſt with a loud noiſe, like the firing of cannons, and filled the room where I was with ſmoke and the ſmell of ſulphur. I was fortunately three or four feet out of the line in which it moved. I was however ſtruck againſt the wall near which I ſtood; my body was covered with fire, and I thought for ſome time I ſhould have been ſuffocated with ſmoke and the ſmell of ſulphur; but by the bleſſing of Providence I eſcaped unhurt, and my houſe received no damage.

I reſpectfully am,

SIR,

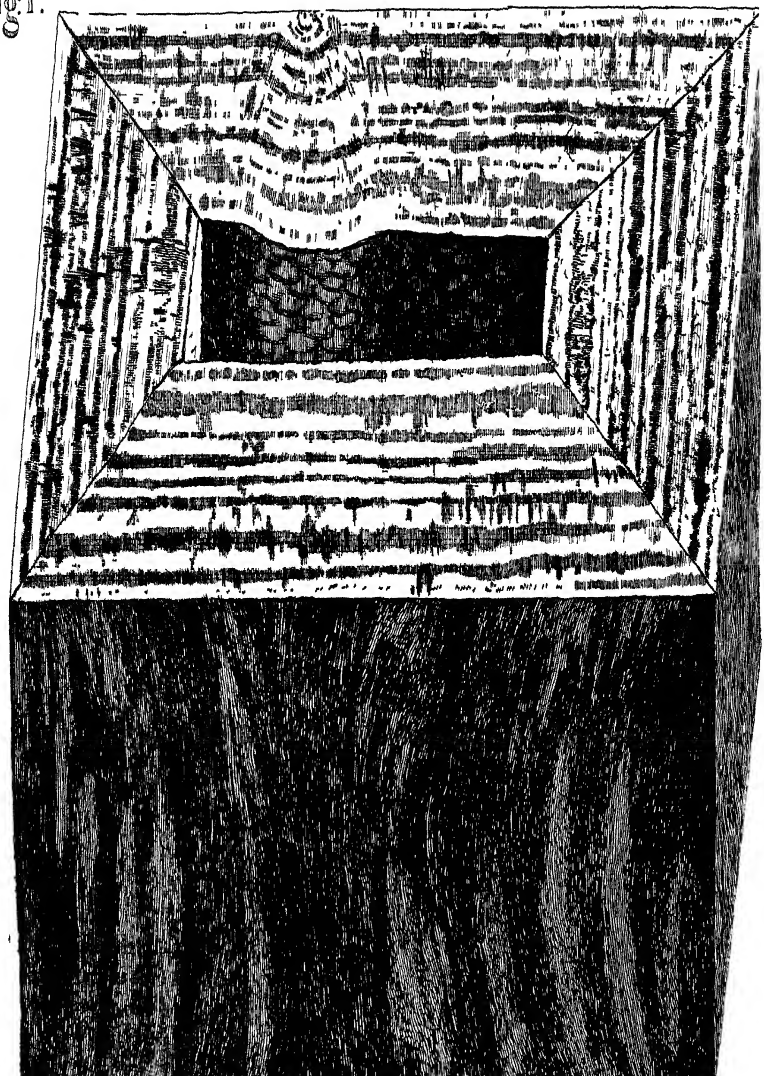
Your moſt humble Servant,

Holt, Auguſt 22, 1772. William Paradiſe.

XXX. *A*

B

Fig. 1.



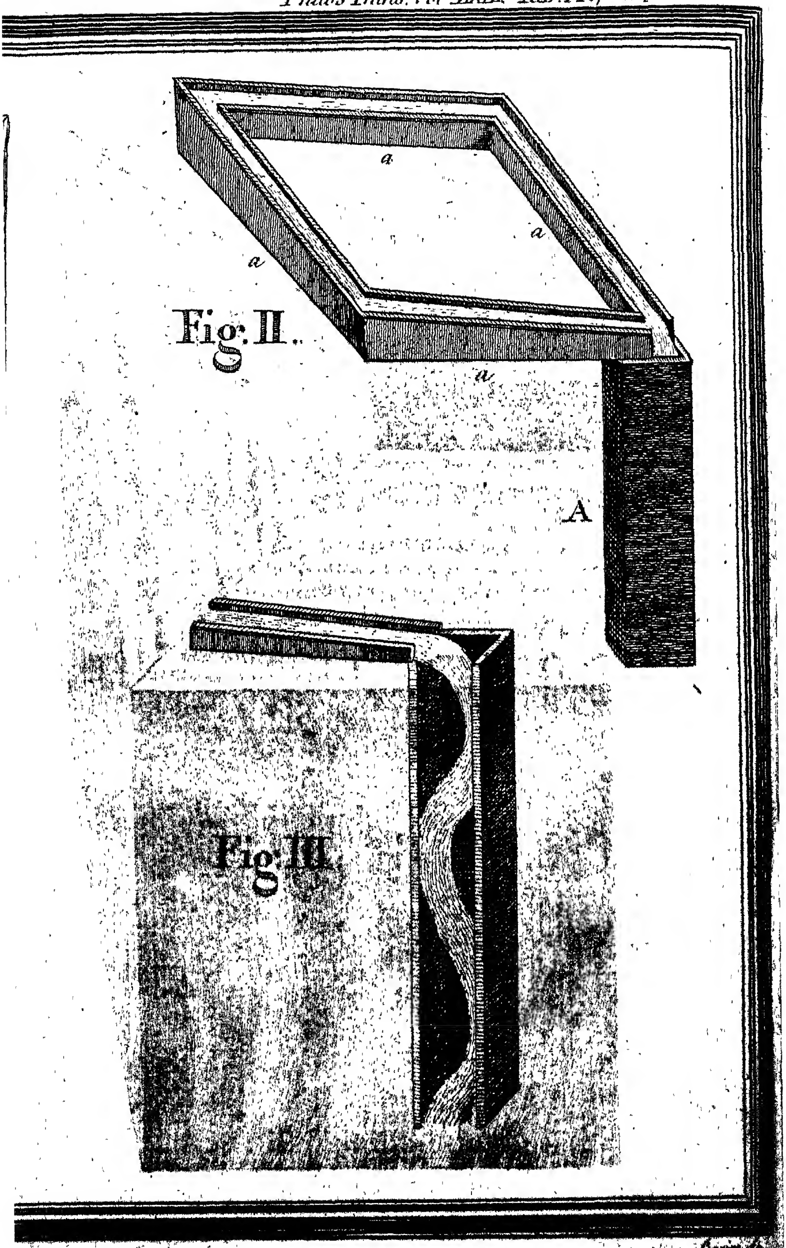

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Fig. II.
A
Fig. III.

XXX. *A Letter to* Mathew Maty, *M. D. Sec. R. S.*; *containing ſome Obſervations on a ſingular Sparry Incruſtation found in* Somerſetſhire. *By* Edward King, *Eſquire, F. R. S.*

John Street, Dec. 18, 1772.

SIR,

Read March 25, 1773.

THE ſummer before laſt, when I was in Somerſetſhire, I received a preſent of a very curious foſſil from the Rev. Mr. Catcott of Briſtol, a gentleman who has taken much pains to collect the foſſil bodies of that County, and is very indefatigable in his reſearches. And as ſome few ſingular obſervations occur to me with regard to this extraordinary maſs, I take the liberty to trouble you with them; and requeſt, if you think them at all deſerving any conſideration, that they may be laid before the Society, together with a drawing of the foſſil, (Tab. X.) and with the very exact account I received of its production from Mr. Catcott.

In the pariſh of High Littleton, in the County of Somerſet, ſituated about midway between Briſtol and Wells, are ſeveral Coal-mines; and about the end of the year 1766, a new ſhaft (or pit) was

opened,

for the purpoſe of conveying air into an adjoining work, called Mearn's coal-pit; but when this ſhaft was finiſhed, the water that flowed in from the ſides, and which at firſt was taken up by buckets, greatly incommoded the under-works; and therefore, to prevent this inconvenience, the miners, at about the depth of ten fathom, and juſt below the place where the water broke in, affixed to the four ſides of the pit ſome wooden ſhoots (as repreſented at Fig. 2.), about four or five inches wide, and as many deep; all of them a little inclined towards one corner, where was a hollow perpendicular pipe or trunk of elm, nearly a long ſquare (as repreſented at A), being about ſeven inches and an half one way, and four inches and an half the other; and through this the water, that fell into the lateral ſhoots, was conveyed down to the level (or paſſage out); which being about ſeven fathoms lower than the ſhoots, the hollow perpendicular trunk was about fourteen yards in length.

This trunk having been thus fixed up, in the latter end of the year 1766, was in about three years time, or rather leſs, found to be much obſtructed, and ſtopped up; ſo that, in Auguſt 1769, the miners were obliged to take it up: and then, on examining it, and taking it to pieces, they found the whole cavity, from one end to the other, nearly filled with a ſort of ſparry incruſtation, ſomewhat ſofter than marble, but harder than alabaſter, and which therefore I ſhall venture to call a ſpecies of marble. And the ſpecimen now laid before the Society, and repreſented by the drawing (Fig. 1.), and alſo another ſpecimen of the like ſort, preſented to the Society,

are merely tranſverſe ſections of the ſubſtance, with which the pipe was filled.

The water, that flowed into the pit on all ſides, iſſued from a ſtratum of hard brown and reddiſh ſand-ſtone, replete with ſhining ſparry *micæ*, and ſome ocherous matter; and had, in its paſſage through the trunk, regularly filled up the cavity, by ſlow degrees, with ſolid incruſtations; inſomuch, that the increaſe of the marble is marked much in the ſame manner as the increaſe of the growth of a tree appears to be, when the trunk of it is cut horizontally: and at laſt the water had left only the cavity, which now appears in the middle of the block, and which was uniform in its figure from one end of the pipe to the other, and nearly ſimilar to the original cavity; but which, at laſt, not being large enough to let all the water paſs, occaſioned the diſcovery. Since that time, in order to prevent the inconvenience (if poſſible) a new trunk has been made, larger than the firſt; and yet, in June 1771, this new trunk alſo was ſo far filled up with the ſparry incruſtation, that there was but juſt room to thruſt four fingers into the central cavity; and the lateral ſhoots, or troughs, alſo have filled ſo faſt, that they have been obliged, every now and then, to clean them out.

This, Sir, is the hiſtory of the ſpecimen now laid before the Society, and of the Mine from whence it came; and the obſervations I would beg leave to make, are the following.

1ſt, As the water flowed in from the ſhoots, on two ſides of the ſquare trunk or pipe, it is manifeſt that the ſtreams muſt have ſtricken againſt each

 other,

other, at the corner of the pipe where they firſt met, and alſo at the oppoſite corner. And, as it is a known principle of mechanics, that a body, which is acted upon by two forces moving in different directions, will deſcribe the diagonal of a parallelogram, of which the directions of thoſe forces ſhall be the ſides; ſo here, the line in which the two ſtreams met, and impeded each other's motion, has plainly, as the marble increaſed, gone on in the diagonal of ſuch a parallelogram from both the corners; viz. from that where the pipe joined the ſhoots, or troughs, and from the oppoſite one: but it is alſo very remarkable, that there is ſuch a diagonal line, not only at theſe corners, but in like manner at the other two; which can be accounted for no otherwiſe, than by ſuppoſing that each of the two ſtreams, daſhing againſt the oppoſite ſide of the pipe, formed continually, the whole way down, another ſtream, in a contrary direction, as repreſented in Fig. 3; and ſo, both together, produced the ſame effect throughout the whole pipe, as if there had been four ſtreams flowing over the four ſides. Upon examining the block, however, very ſtrictly, it appears, that the lines in the diagonal one way, are ſtronger than thoſe in the diagonal the other way; and indeed the ſpecimen of the pipe, preſented to the Society, has even broken in halves, exactly in one of the diagonals, though the block here deſcribed remains intire, and has the appearance of having had its ſides joined accurately, in the manner in which a ſkilful workman would fit four boards to glued together.

2dly.

2dly, At the place marked B, Fig. 1, there ſeems to have been, by ſome accident or other, the point of a ſmall nail projecting into the pipe; and here, it is very remarkable, that, either by the daſhing of the water, or rather perhaps by an effect which iron has been obſerved to have of haſtening and increaſing petrification, the incruſtation has gone on faſter than in other parts of the ſame ſide; but ſo regularly, that, from the point of the nail to the inner cavity, there is a ſwelling, or protuberance, ſo uniform, that it makes throughout nearly the ſame ſegment of different circles, of which the point of the nail is the common center; and that not merely directly oppoſite to the nail, but throughout this whole block and even further downwards.

3dly, The regular increaſe of theſe ſegments of circles is viſible in each *lamina* of the block (if I may be allowed that expreſſion), and in each *lamina* the diameter of the circle increaſes in due proportion; ſo that it is ſtill nearly the ſame ſegment; though, if there be any difference, it is rather a ſmaller portion of a larger circle; as, from the cauſe which occaſioned it, one would be led to expect. And with regard to theſe *laminæ*, it is worth obſerving, that as they mark the increaſe of the marble uniformly all round, as the growth of a tree is marked (only the marble increaſed inward, whereas a tree grows outward) ſo they ſeem to have become viſible, and to have been thus diſtinctly marked, by means of the water bringing, at different times, more or leſs oker along with the ſparry matter: and this is the more probable, as the whole country all round abounds

 with

with beds of oker, and the waters are ſometimes much tinged with it.

4thly, The cavity left in the middle of the block is not perfectly ſimilar to the original cavity of the trunk, or pipe; becauſe the water did not flow quite uniformly over the edges, at the ends of the ſhoots or troughs, in conſequence, probably, of their not lying exactly horizontally: whence, more water fell upon and againſt one part of the ſides of the trunk, than againſt the other.

5thly, The outſide of the block has taken off impreſſions of all the roughneſſes, knots, and ſhivers of the elm boards, which compoſed the trunk or pipe, even more accurately than they could have been taken off by wax, plaiſter of Paris, or almoſt any compoſition whatever, and certainly much more durably: which impreſſions, although they are not ſo well repreſented in the drawing, in conſequence of their fineneſs, yet appear ſufficiently plain, both on the ſpecimen here deſcribed, and on that preſented to the Society, and are exceedingly well deſerving of notice.

There is in the Philoſophical Tranſactions (Vol.LX. p. 47.) a very curious paper, from our learned foreign Member R. E. Raſpe, concerning the production of white marble in a ſimilar manner; in which paper he mentions the taking off impreſſions of medallions, by means of petrifying waters. And I remember a paper was read at the Royal Society ſome time ago, containing an account of ſeveral impreſſions, actually ſo taken off in a ſhort time, in durable marble, by means of a petrifying water, near Bologna in Italy: when ſome of the impreſſions were alſo ſent, both to the

the Royal Society, and to the Britiſh Muſeum. And, as this block here deſcribed, and the whole contents of the pipe, of above forty feet in length, were formed in leſs than three years, there is reaſon to conclude, that the water of this Mine in Somerſetſhire is as capable of being improved to the purpoſes of a new manufactory, as either that near Bologna, or thoſe of Germany and Bohemia. And it is perhaps worth mentioning, that ſomething of this ſort has actually been attempted, with good ſucceſs, in Peru; for we are told by P. Feuillée (who made ſeveral curious obſervations in South America, both phyſiological and aſtronomical, in 1709), that he ſaw many ſtatues and beautiful vaſes (or holy water pots), in the churches at Lima, which were ſimply caſt in moulds, by means alone of a petrifying water near Guankabalika, or Guankavelika. And this circumſtance is alſo mentioned (p. 236.), in a Deſcription of Peru, publiſhed in 1748, a great part of which is taken from Feuillée's account.

6thly, This block of marble takes a very fine poliſh, as appears by the ſpecimen, the ſections of which are poliſhed; and if caſts of medals, or other things, were taken in ſmooth moulds, well formed, their ſurfaces would, therefore, probably appear well poliſhed, as thoſe of the medals did, which came from Bologna.

7thly and laſtly, I would only add, that Dr. Pococke, in his Travels (Vol. II. p. 264.), deſcribing a very curious grotto in the iſland of Candia, or Crete, which exceeded all others that he ever ſaw in beauty, and the ſlenderneſs of the pillars, one of which is near twenty feet high, and even tranſparent,

ſays,

ſays, " As I had ſeen ſtones of this kind hewn out " of a rock at Mount Lebanon, which were uſed as " white marble, and appeared to be alabaſter, this " made me imagine, that when theſe ſorts of petri- " factions are hard enough to receive a poliſh, they " then become the oriental tranſparent alabaſter, " which is ſo much valued, and of which there are " two curious columns at the high altar of St. Mark " in Venice." Perhaps Dr. Pococke does not here ſufficiently diſtinguiſh between marble and alabaſter; but I add his remark, merely to ſhew how valuable theſe incruſtations may become, and how much they deſerve not to be neglected.

I am,

S I R,

Very reſpectfully,

Your moſt obedient,

humble ſervant,

Edward King.

XXXI. *Experiments and Obſervations on the Singing of Birds, by the Hon.* Daines Barrington, *Vice Pref. R. S. In a Letter to* Mathew Maty, *M. D. Sec. R. S.*

Read April 22, May 6, and May 13, 1773.

January 10, 1773.

DEAR SIR,

AS the experiments and obſervations I mean to lay before the Royal Society relate to the ſinging of birds, which is a ſubject that hath never before been ſcientifically treated of *, it may not be improper to prefix an explanation of ſome uncommon terms, which I ſhall be obliged to uſe, as well as others which I have been under a neceſſity of coining.

To *chirp*, is the firſt ſound which a young bird utters, as a cry for food, and is different in all neſtlings, if accurately attended to; ſo that the hearer may diſtinguiſh of what ſpecies the birds are, though the neſt may hang out of his ſight and reach.

* Kircher, indeed, in his *Muſurgia*, hath given us ſome few paſſages in the ſong of the nightingale, as well as the call of a quail and cuckow, which he hath engraved in muſical characters. Theſe inſtances, however, only prove that ſome birds have in their ſong, notes which correſpond with the intervals of our common ſcale of the muſical octave.

This

This cry is, as might be expected, very weak and querulous; it is dropped entirely as the bird grows ſtronger, nor is afterwards intermixed with its ſong, the *chirp* of a nightingale (for example) being hoarſe and diſagreeable.

To this definition of the *chirp*, I muſt add, that it conſiſts of a ſingle ſound, repeated at very ſhort intervals, and that it is common to neſtlings of both ſexes.

The *call* of a bird, is that ſound which it is able to make, when about a month old; it is, in moſt inſtances (which I happen to recollect), a repetition of one and the ſame note, is retained by the bird as long as it lives, and is common, generally, to both the cock and hen *.

The next ſtage in the notes of a bird is termed, by the bird-catchers, *recording*, which word is probably derived from a muſical inſtrument, formerly uſed in England, called a recorder †.

This attempt in the neſtling to ſing, may be compared to the imperfect endeavour in a child to babble. I have known inſtances of birds beginning to *record* when they were not a month old.

* For want of terms to diſtinguiſh the notes of birds, Bellon applies the verb *chantent*, or ſing, to the gooſe and crane, as well as the nightingale. "Pluſieurs oiſeaux *chantent* la nuit, comme "eſt l'oye, la grue, & le roſſignol." Bellon's Hiſt. of Birds, p. 50.

† It ſeems to have been a ſpecies of flute, and was probably uſed to teach young birds to pipe tunes.

Lord Bacon deſcribes this inſtrument to have been ſtrait, to have had a leſſer and greater bore, both above and below, to have required very little breath from the blower, and to have had what he calls a *fipple*, or ſtopper. See his ſecond Century of Experiments.

This

This first essay does not seem to have the least rudiments of the future song; but as the bird grows older and stronger, one may begin to perceive what the nestling is aiming at.

Whilst the scholar is thus endeavouring to form his song, when he is once sure of a passage, he commonly raises his tone, which he drops again when he is not equal to what he is attempting; just as a singer raises his voice, when he not only recollects certain parts of a tune with precision, but knows that he can execute them.

What the nestling is not thus thoroughly master of, he hurries over, lowering his tone, as if he did not wish to be heard, and could not yet satisfy himself.

I have never happened to meet with a passage in any writer, which seems to relate to this stage of singing in a bird, except, perhaps, in the following lines of Statius:

> " ———Nunc volucrum novi
> " Questus, inexpertumque carmen,
> " Quod tacitâ statuere brumâ."
>
> Stat. Sylv. L. iv. Ecl. 5.

A young bird commonly continues to *record* for ten or eleven months, when he is able to execute every part of his song, which afterwards continues fixed, and is scarcely ever altered.

When the bird is thus become perfect in his lesson, he is said to *sing his song round*, or in all its varieties of passages, which he connects together, and executes without a pause.

I would therefore define a bird's *ſong* to be a ſucceſſion of three or more different notes, which are continued without interruption during the ſame interval with a muſical bar of four crotchets in an adagio movement, or whilſt a pendulum ſwings four ſeconds.

By the firſt requiſite in this definition, I mean to exclude the call of a cuckow, or *clucking* of a hen *, as they conſiſt of only two notes; whilſt the ſhort burſts of ſinging birds, contending with each other (called *jerks* by the bird-catchers), are equally diſtinguiſhed from what I term *ſong*, by their not continuing for four ſeconds.

As the notes of a cuckow and hen, therefore, though they exceed what I have defined the *call* of a bird to be, do not amount to its *ſong*, I will, for this reaſon, take the liberty of terming ſuch a ſucceſſion of two notes as we hear in theſe birds, the *varied call*.

Having thus ſettled the meaning of certain words, which I ſhall be obliged to make uſe of, I ſhall now proceed to ſtate ſome general principles with regard to the ſinging of birds, which ſeem to reſult from the experiments I have been making for ſeveral years, and under a great variety of circumſtances.

Notes in birds are no more innate, than language is in man, and depend entirely upon the maſter under which they are bred, as far as their organs will enable them to imitate the ſounds which they have frequent opportunities of hearing.

* The common hen, when ſhe lays, repeats the ſame note very often, and concludes with the ſixth above, which ſhe holds for a longer time.

I

Moſt

Moſt of the experiments I have made on this ſubject have been made with cock linnets, which were fledged and nearly able to leave their neſt, on account not only of this bird's docility, and great powers of imitation, but becauſe the cock is eaſily diſtinguiſhed from the hen at that early period, by the ſuperior whiteneſs in the wing *.

In many other ſorts of ſinging birds the male is not at the age of three weeks ſo certainly known from the female; and if the pupil turns out to be a hen,

" ——— ibi omnis
" Effuſus labor."

The Greek poets made a ſongſter of the τεττιξ, whatever animal that may be, and it is remarkable that they obſerved the female was incapable of ſinging as well as hen birds:

Ειτ' εισιν οι τεττιγες ϐκ ευδαιμονες,
Ων ταις γυναιξιν ϐ δ'οτιϐν φωνης ενι;
Comicorum Græcorum Sententiæ, p. 452.
Ed. Steph.

I have indeed known an inſtance or two of a hen's making out ſomething like the ſong of her ſpecies; but theſe are as rare as the common hen's being heard to crow.

I rather ſuſpect alſo, that thoſe parrots, magpies, &c. which either do not ſpeak at all, or very little, are hens of thoſe ſpecies.

* The white reaches almoſt to the ſhaft of the quill feathers, and in the hen does not exceed more than half.

 I have

I have educated neſtling linnets under the three beſt ſinging larks, the *ſkylark*, *woodlark*, and *titlark*, every one of which, inſtead of the linnet's ſong, adhered entirely to that of their reſpective inſtructors.

When the note of the *titlark-linnet* * was thoroughly *fixed*, I hung the bird in a room with two common linnets, for a quarter of a year, which were full in ſong; the *titlark-linnet*, however, did not borrow any paſſages from the linnet's ſong, but adhered ſtedfaſtly to that of the titlark.

I had ſome curioſity to find out whether an European neſtling would equally learn the note of an African bird: I therefore educated a young linnet under a *vengolina* †, which imitated its African maſter ſo exactly, without any mixture of the linnet ſong, that it was impoſſible to diſtinguiſh the one from the other.

This *vengolina-linnet* was abſolutely perfect, without ever uttering a ſingle note by which it could have been known to be a linnet. In ſome of my other experiments, however, the neſtling linnet retained the *call* of its own ſpecies, or what the bird-

* I thus call a bird which ſings notes he would not have learned in a wild ſtate; thus by a *ſkylark-linnet*, I mean a linnet with the ſkylark ſong; a *nightingale robin*, a robin with the nightingale ſong, &c.

† This bird ſeems not to have been deſcribed by any of the ornithologiſts; it is of the *finch* tribe, and about the ſame ſize with our aberdavine (or ſiſkin). The colours are grey and white, and the cock hath a bright yellow ſpot upon the rump. It is a very familiar bird, and ſings better than any of thoſe which are not European, except the American *mocking bird*.

catchers

catchers term the linnet's *chuckle*, from ſome reſemblance to that word when pronounced.

I have before ſtated, that all my neſtling linnets were three weeks old, when taken from the neſt; and by that time they frequently learn their *own call* from the parent birds, which I have mentioned to conſiſt of only a ſingle note.

To be certain, therefore, that a neſtling will not have even the *call* of its ſpecies, it ſhould be taken from the neſt when only a day or two old; becauſe, though neſtlings cannot ſee till the ſeventh day, yet they can hear from the inſtant they are hatched, and probably, from that circumſtance, attend to ſounds, more than they do afterwards, eſpecially as the call of the parents announces the arrival of their food.

I muſt own, that I am not equal myſelf, nor can I procure any perſon to take the trouble of breeding up a bird of this age, as the odds againſt its being reared are almoſt infinite. The warmth indeed of incubation may be, in ſome meaſure, ſupplied by cotton and fires; but theſe delicate animals require, in this ſtate, being fed almoſt perpetually, whilſt the nouriſhment they receive ſhould not only be prepared with great attention, but given in very ſmall portions at a time.

Though I muſt admit, therefore, that I have never reared myſelf a bird of ſo tender an age, yet I have happened to ſee both a linnet and a goldfinch which were taken from their neſts when only two or three days old.

The firſt of theſe belonged to Mr. Matthews, an apothecary at Kenſington, which, from a want of other

other ſounds to imitate, almoſt articulated the words *pretty boy*, as well as ſome other ſhort ſentences: I heard the bird myſelf repeat the words *pretty boy*; and Mr. Matthews aſſured me, that he had neither the note or call of any bird whatſoever.

This talking linnet died laſt year, and many people went from London to hear him ſpeak.

The goldfinch I have before mentioned, was reared in the town of Knighton in Radnorſhire, which I happened to hear, as I was walking by the houſe where it was kept.

I thought indeed that a *wren* was ſinging; and I went into the houſe to inquire after it, as that little bird ſeldom lives long in a cage.

The people of the houſe, however, told me, that they had no bird but a goldfinch, which they conceived to ſing its own natural note, as they called it; upon which I ſtaid a conſiderable time in the room, whilſt its notes were merely thoſe of a *wren*, without the leaſt mixture of goldfinch.

On further inquiries, I found that the bird had been taken from the neſt when only two or three days old, that it was hung in a window which was oppoſite to a ſmall garden, whence the neſtling had undoubtedly acquired the notes of the wren, without having had any opportunity of learning even the *call* of the goldfinch.

Theſe facts which I have ſtated ſeem to prove very deciſively, that birds have not any innate ideas of the notes which are ſuppoſed to be peculiar to each ſpecies. But it will poſſibly be aſked, why in a wild ſtate they adhere ſo ſteadily to the ſame ſong, in

in ſo much that it is is well known, before the bird is heard, what notes you are to expect from him.

This, however, ariſes entirely from the neſtling's attending only to the inſtruction of the parent bird, whilſt it diſregards the notes of all others, which may perhaps be ſinging round him.

Young Canary-birds are frequently reared in a room where there are many other ſorts; and yet I have been informed that they only learn the ſong of the parent cock.

Every one knows, that the common houſe-ſparrow, when in a wild ſtate, never does any thing but chirp: this, however, does not ariſe from want of powers in this bird to imitate others; but becauſe he only attends to the parental note.

But, to prove this deciſively, I took a common Sparrow from the neſt when it was fledged, and educated him under a linnet: the bird, however, by accident heard a goldfinch alſo, and his ſong was, therefore, a mixture of the linnet and goldfinch.

I have tried ſeveral experiments, in order to obſerve from what circumſtances birds fix upon any particular note when taken from the parents; but cannot ſettle this with any ſort of preciſion, any more than at what period of their *recording* they determine upon the ſong to which they will adhere.

I educated a young robin under a very fine Nightingale; which, however, began already to be out of ſong, and was perfectly mute in leſs than a fortnight.

This robin afterwards ſung three parts in four *nightingale*; and the reſt of his ſong was what the bird-

bird-catchers call *rubbiſh*, or no particular note whatſoever.

I hung this robin nearer to the nightingale than to any other bird; from which firſt experiment I conceived, that the ſcholar would imitate the maſter which was at the leaſt diſtance from him.

From ſeveral other experiments, however, which I have ſince tried, I find it to be very uncertain what notes the neſtling will moſt attend to, and often their ſong is a mixture; as in the inſtance which I before ſtated of the ſparrow.

I muſt own alſo, that I conceived, from the experiment of educating the robin under a nightingale, that the ſcholar would fix upon the note which it firſt heard when taken from the neſt; I imagined likewiſe, that, if the nightingale had been fully in ſong, the inſtruction for a fortnight would have been ſufficient.

I have, however, ſince tried the following experiment, which convinces me, ſo much depends upon circumſtances, and perhaps caprice in the ſcholar, that no general inference, or rule, can be laid down with regard to either of theſe ſuppoſitions.

I educated a neſtling robin under a woodlark-linnet, which was full in ſong, and hung very near to him for a month together: after which, the robin was removed to another houſe, where he could only hear a ſkylark-linnet. The conſequence was that the neſtling did not ſing a note of woodlark (though I afterwards hung him again juſt above the wood-lark-linnet) but adhered entirely to the ſong of the ſkylark-linnet.

Having

Having thus ſtated the reſult of ſeveral experiments, which were chiefly intended to determine, whether birds had any innate ideas of the notes, or ſong, which is ſuppoſed to be peculiar to each ſpecies, I ſhall now make ſome general obſervations on their ſinging; though perhaps the ſubject may appear to many a very minute one.

Every poet, indeed, ſpeaks with raptures of the harmony of the groves; yet thoſe even, who have good muſical ears, ſeem to pay little attention to it, but as a pleaſing noiſe.

I am alſo convinced (though it may ſeem rather paradoxical), that the inhabitants of London diſtinguiſh more accurately, and know more on this head, than of all the other parts of the iſland taken together.

This ſeems to ariſe from two cauſes.

The firſt is, that we have not more muſical ideas which are innate, than we have of language; and therefore thoſe even, who have the happineſs to have organs which are capable of receiving a gratification from this ſixth ſenſe (as it hath been called by ſome) require, however, the beſt inſtruction.

The orcheſtra of the opera, which is confined to the metropolis, hath diffuſed a good ſtile of playing over the other bands of the capital, which is, by degrees, communicated to the fidler and ballad-ſinger in the ſtreets; the organs in every church, as well as thoſe of the Savoyards, contribute likewiſe to this improvement of muſical faculties in the Londoners.

If the ſinging of the ploughman in the country is therefore compared with that of the London black-

guard, the ſuperiority is infinitely on the ſide of the latter; and the ſame may be obſerved in comparing the voice of a country girl and London houſe-maid, as it is very uncommon to hear the former ſing tolerably in tune.

I do not mean by this, to aſſert that the inhabitants of the country are not born with as good muſical organs; but only, that they have not the ſame opportunities of learning from others, who play in tune themſelves.

The other reaſon for the inhabitants of London judging better in relation to the ſong of birds, ariſes from their hearing each bird ſing diſtinctly, either in their own or their neighbours ſhops; as alſo from a bird continuing much longer in ſong whilſt in a cage, than when at liberty; the cauſe of which I ſhall endeavour hereafter to explain.

Thoſe who live in the country, on the other hand, do not hear birds ſing in their woods for above two months in the year, when the confuſion of notes prevents their attending to the ſong of any particular bird; nor does he continue long enough in a place, for the hearer to recollect his notes with accuracy.

Beſides this, birds in the ſpring ſing very loud indeed; but they only give ſhort jerks, and ſcarcely ever the whole compaſs of their ſong.

For theſe reaſons, I have never happened to meet with any perſon, who had not reſided in London, whoſe judgement or opinion on this ſubject I could the leaſt rely upon; and a ſtronger proof of this cannot be given, than that moſt people, who keep Ca-

nary

nary-birds do not know that they ſing chiefly either the titlark, or nightingale notes *.

Nothing, however, can be more marked than the note of a nightingale called its *jug*, which moſt of the Canary-birds brought from the Tyrol commonly have, as well as ſeveral nightingale *ſtrokes*, or particular paſſages in the ſong of that bird.

I mention this ſuperior knowledge in the inhabitants of the capital, becauſe I am convinced, that, if others are conſulted in relation to the ſinging of birds, they will only miſlead, inſtead of giving any material or uſeful information †.

Birds in a wild ſtate do not commonly ſing above ten weeks in the year; which is then alſo confined to the cocks of a few ſpecies; I conceive, that this laſt

* I once ſaw two of theſe birds which came from the Canary iſlands; neither of which had any ſong at all; and I have been informed, that a ſhip brought a great many of them not long ſince, which ſung as little.

Moſt of thoſe Canary-birds, which are imported from the Tyrol, have been educated by parents, the progenitor of which was inſtructed by a nightingale; our Engliſh Canary-birds have commonly more of the titlark note.

The traffick in theſe birds makes a ſmall article of commerce, as four Tyroleze generally bring over to England ſixteen hundred every year; and though they carry them on their backs one thouſand miles, as well as pay 20l. duty for ſuch a number, yet upon the whole it anſwers to ſell theſe birds at 5s. a piece.

The chief place for breeding Canary-birds is Inſpruck and its environs, from whence they are ſent to Conſtantinople, as well as every part of Europe.

† As it will not anſwer to catch birds with clap-nets any where but in the neighbourhood of London, moſt of the birds which may be heard in a country town are neſtlings, and conſequently cannot ſing the ſuppoſed natural ſong in any perfection.

 circumſtance

circumſtance ariſes from the ſuperior ſtrength of the muſcles of the larynx.

I procured a cock nightingale, a cock and hen blackbird, a cock and hen rook, a cock linnet, as alſo a cock and hen chaffinch, which that very eminent anatomiſt, Mr. Hunter, F. R. S. was ſo obliging as to diſſect for me, and begged, that he would particularly attend to the ſtate of the organs in the different birds, which might be ſuppoſed to contribute to ſinging.

Mr. Hunter found the muſcles of the larynx to be ſtronger in the nightingale than in any other bird of the ſame ſize; and in all thoſe inſtances (where he diſſected both cock and hen) that the ſame muſcles were ſtronger in the cock.

I ſent the cock and hen rook, in order to ſee whether there would be the ſame difference in the cock and hen of a ſpecies which did not ſing at all. Mr. Hunter, however, told me, that he had not attended ſo much to their comparative organs of voice, as in the other kinds; but that, to the beſt of his recollection, there was no difference at all.

Strength, however, in theſe muſcles, ſeems not to be the only requiſite; the birds muſt have alſo great plenty of food, which ſeems to be proved ſufficiently by birds in a cage ſinging the greateſt part of the year, when the wild ones do not (as I obſerved before) continue in ſong above ten weeks.

The food of ſinging birds conſiſts of plants, inſects, or ſeeds, and of the two firſt of theſe there is infinitely the greateſt profuſion in the ſpring.

As for ſeeds, which are to be met with only in the autumn, I think they cannot well find any great

quantities

quantities of them in a country ſo cultivated as England is; for the ſeeds in meadows are deſtroyed by mowing; in paſtures, by the bite of the cattle; and in arable, by the plough, when moſt of them are buried too deep for the bird to reach them *.

I know well that the ſinging of the cock-bird in the ſpring is attributed by many † to the motive only of pleaſing its mate during incubation.

Thoſe, however, who ſuppoſe this, ſhould recollect, that much the greater part of birds do not ſing at all: why ſhould their mate therefore be deprived of this ſolace and amuſement?

The bird in a cage, which, perhaps, ſings nine or ten months in a year, cannot do ſo from this inducement; and, on the contrary, it ariſes chiefly from contending with another bird, or indeed againſt almoſt any ſort of continued noiſe.

Superiority in ſong gives to birds a moſt amazing aſcendency over each other; as is well known to the bird-catchers by the faſcinating power of their call-birds, which they contrive ſhould moult prematurely for this purpoſe.

But, to ſhew deciſively that the ſinging of a bird in the ſpring does not ariſe from any attention to its mate, a very experienced catcher of nightingales hath informed me, that ſome of theſe birds have *jerked* the inſtant they were caught. He hath alſo brought

* The plough indeed may turn up ſome few ſeeds, which may ſtill be in an eatable ſtate.

† See, amongſt others, M. de Buffon, in his lately-publiſhed Ornithology.

to me a nightingale, which had been but a few hours in a cage, and which burst forth in a roar of song.

At the same time this bird is so sulky on its first confinement, that he must be crammed for seven or eight days, as he will otherwise not feed himself: it is also necessary to tye his wings, to prevent his killing himself against the top or sides of the cage.

I believe there is no instance of any bird's singing which exceeds our blackbird in size; and possibly this may arise from the difficulty of its concealing itself, if it called the attention of its enemies, not only by bulk, but by the proportionable loudness of its notes *.

I should rather conceive, it is for the same reason that no hen-bird sings, because this talent would be still more dangerous during incubation; which may possibly also account for the inferiority in point of plumage.

I shall now consider how far the singing of birds resembles our known musical intervals, which are never marked more minutely than to half notes; because, though we can form every gradation from half-note to half-note, by drawing the finger gently over the string of a violin, or covering by degrees the hole of a flute; yet we cannot produce such a minute interval at command, when a quarter-note for example might be required.

Ligon, indeed, in his history of Barbadoes, hath the following passage: "The next bird is of the co-

* For the same reason, most large birds are wilder than the smaller ones.

"lour

" lour of the fieldfare; but the head is too large " for the body; and for that reafon fhe is called a " counfellor. She performs that with her voice, " which no inftrument can play, or voice can fing; " and that is quarter-notes, her fong being compofed " of them, and every one a note higher than another." Hift. Barb. p. 60.

Ligon appears, from other parts of his work, to have been mufical; but I fhould doubt much whether he was quite fure of thefe quarter intervals, fo as to fpeak of them with precifion.

Some paffages of the fong in a few kinds of birds correfpond with the intervals of our mufical fcale (of which the cuckow is a ftriking and known inftance): much the greater part, however, of fuch fong is not capable of mufical notations.

This arifes from three caufes: the firft is, that the rapidity is often fo great, and it is alfo fo uncertain when they may ftop, that we cannot reduce the paffages to form a mufical bar, in any time whatfoever.

The fecond is, that the pitch of moft birds is confiderably higher * than the moft fhrill notes of thofe inftruments which contain even the greateft compafs.

* Dr Wallis is miftaken in part of what he fuppofes to be the caufe of fhrillnefs in the voice, " Nam ut tubus, fic trachea " longior, & ftrictior, fonum efficit magis acutum." Grammar, p. 3.

The narrower the pipe is, the more fharp the pitch as he rightly obferves; but the length of the tube hath juft the contrary effect, becaufe players on the flute always infert a longer middle-piece, when they want to make the inftrument more flat.

I have

I have before ſaid, that our ideas of a voice, or inſtrument, being perfectly in tune or not, ariſe from comparing it with the muſical intervals to which we are moſt accuſtomed.

As the upper and lower parts of every inſtrument, however, are but ſeldom uſed, we are not ſo well acquainted with the intervals in the higheſt and loweſt octaves, as we are with thoſe which are more central; and for this reaſon the harpſichord-tuners find it more difficult to tune theſe extreme parts.

As a bird's pitch, therefore, is higher than that of any inſtrument, we are conſequently at a ſtill greater loſs when we attempt to mark their notes in muſical characters, which we can ſo readily apply to ſuch as we can diſtinguiſh with preciſion.

The third, however, and unſurmountable difficulty is, that the intervals uſed by birds are commonly ſo minute, that we cannot judge at all of them from the more groſs intervals into which we divide our muſical octave.

It ſhould therefore be recollected, by thoſe who have contended that the Greeks and Romans were acquainted with ſuch more minute intervals of the octave, that they muſt inſiſt the ancients had organs of ſenſation, with which their degenerate poſterity are totally unprovided.

Though we cannot attain the more delicate and imperceptible intervals in the ſong of birds *, yet many of them are capable of whiſtling tunes with our more groſs intervals, as is well known by the

* There have been inſtances indeed of perſons who would whiſtle the notes of birds, but theſe are too rare to be argued from.

common

common inſtances of piping Bullfinches *, and Canary-birds.

This, however, ariſes from mere imitation of what they hear when taken early from the neſt; for if the inſtrument from which they learn is out of tune, they as readily pipe the falſe, as the true notes of the compoſition.

The next point of compariſon to be made between our muſic and that of birds is, whether they always ſing in the ſame pitch.

This, however, I will not preſume to anſwer with any preciſion, for the reaſon I have before ſuggeſted; I ſhall, however, without reſerve, give the beſt conjectures I can form on this head.

If a dozen ſinging birds of different kinds are heard in the ſame room, there is not any diſagreeable diſſonance (which is not properly reſolved), either to my own ear, or to that of others, whoſe judgement on ſuch a point I can more rely.

At the ſame time, as each bird is ſinging a different ſong, it is extraordinary that what we call harmony ſhould not be perpetually violated, as we experience, in what is commonly called a Dutch concert, when ſeveral tunes are played together.

The firſt requiſite to make ſuch ſounds agreeable to the ear is, that all the birds ſhould ſing in the ſame key, which I am induced to believe that they do, from the following reaſons.

I have long attended to the ſinging of birds, but if I cannot have recourſe to an inſtrument very ſoon,

* Theſe Bullfinches alſo form a ſmall article of commerce, and are chiefly brought from the neighbourhood of Cologne.

I cannot carry the pitch of their notes in my memory, even for a very ſhort time.

I therefore deſired a very experienced harpſichord-tuner (who told me he could recollect any particular note which he happened to hear for ſeveral hours), to mark down when he returned home what he had obſerved on this head.

I have lately received an account from him of the following notes in different birds.

F. natural in woodlarks.

A. natural in common cocks.

C. natural in Bantam cocks.

B. flat in a very large cock.

C. falling to A. commonly in the cuckow.

A. in thruſhes.

D. in ſome owls.

B. flat in ſome others.

Theſe obſervations furniſh five notes, viz. A. B. flat, C. D. and F. to which I can add a ſixth, (viz. G.) from my own obſervations on a nightingale which lived three years in a cage. I can alſo confirm theſe remarks of the harpſichord-tuner by having frequently heard from the ſame bird C. and F.

As one ſhould ſpeak of the pitch of theſe notes with ſome preciſion, the B. flat of the ſpinnet I tried them by, was perfectly in tune with the great bell of St. Paul's.

The following notes, therefore, having been obſerved in different birds, viz. A. B. flat, C. D. F. and G. the E. is only wanting to compleat the ſcale; the ſix other notes, however, afford ſufficient data for making ſome conjectures, at leaſt, with regard to the key in which birds may be ſuppoſed to ſing,

as theſe intervals can only be found in the key of F. with a ſharp third, or that of G. with a flat third.

I muſt own, I ſhould rather ſuppoſe it to be the latter, and for the following reaſons.

Lucretius ſays (and perhaps the conjecture is not only ingenious but well founded), that the firſt muſical notes were learned from birds:

" At liquidas avium voces imitarier ore
" Ante fuit multo, quam lævia carmina cantu
" Concelebrare homines poſſent, cantuque juvare."

Now, of all the muſical tones which can be diſtinguiſhed in birds, thoſe of the cuckow have been moſt attended to, which form a flat third, not only by the obſervations of the harpſichord tuner I have before mentioned, but likewiſe by thoſe of Kircher, in his *Muſurgia.*

I know well that there have been ſome late compoſitions, which introduce the cuckow notes in a ſharp third; theſe compoſers, however, did not trouble themſelves with accuracy in imitating theſe notes, and it anſwered their purpoſe ſufficiently, if there was a general reſemblance.

Another proof of our muſical intervals being originally borrowed from the ſong of birds, ariſes from moſt compoſitions being in a flat third, where muſic is ſimple, and conſiſts merely of melody.

The oldeſt tune I happen to have heard is a Welſh one, called *Morvar Rhydland* *, which is

* Or *Rhydland Marſh*, where the Welſh received a great defeat; Rhydland is in Flintſhire. We find alſo, by the *Orpheus Britannicus*, that even ſo late as the time of Purcel, two parts in the three of his compoſitions are in the flat third.

composed in a flat third; and if the music of the Turks and Chinese is examined in Du Halde and Dr. Shaw, half of the airs are also in a flat third.

The music of two centuries ago is likewise often in a flat third, though ninety-nine compositions out of a hundred are now in the sharp third.

The reason, however, of this alteration seems to be very clear: the flat third is plaintive, and consequently adapted to simple movements, such as may be expected in countries where music hath not been long cultivated.

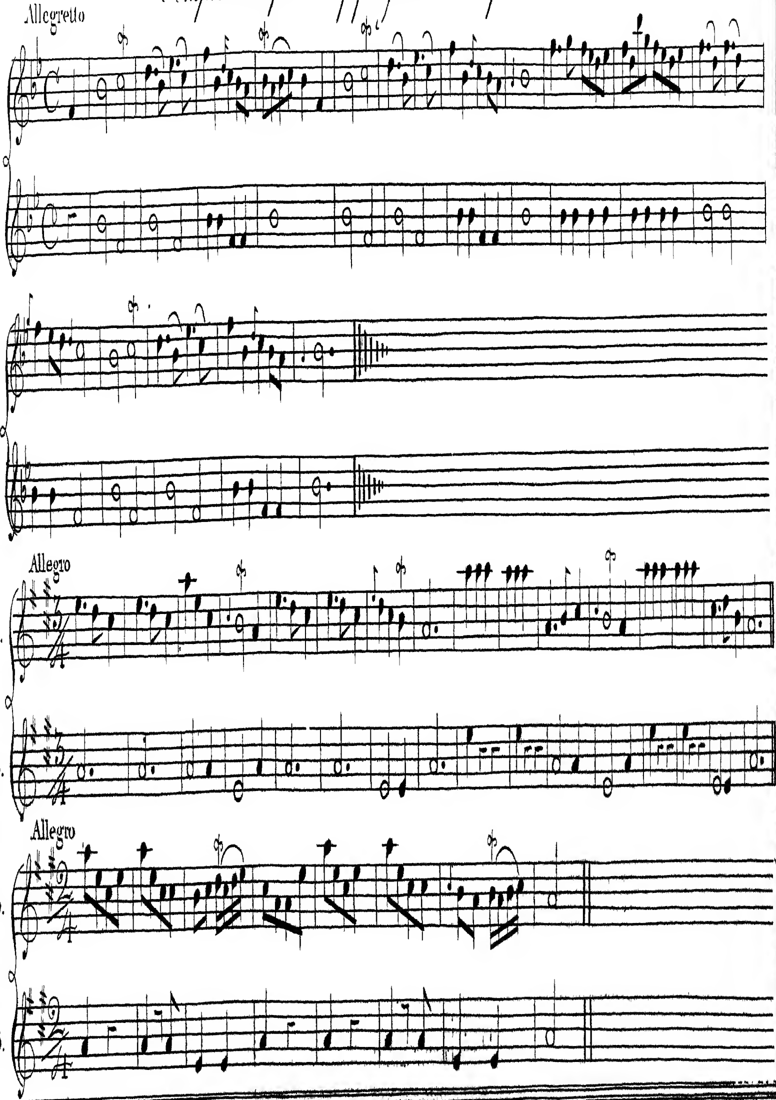
There is on the other hand a most striking brilliancy in the sharp third, which is therefore proper for the amazing improvements in execution, which both singers and players have arrived at within the last fifty years.

When Corelli's music was first published, our ablest violinists conceived that it was too difficult to be performed; it is now, however, the first composition which is attempted by a scholar. Every year also now produces greater and greater prodigies upon other instruments, in point of execution.

I have before observed, that by attending to a nightingale, as well as a robin which was educated under him, I always found that the notes reducible to our intervals of the octave were precisely the same; which is another proof that birds sing always in the same key.

In this circumstance, they differ much from the human singer; because those who are not able to *sing at sight*, often begin a song either above or below the compass of their voice, which they are not therefore able to go through with. As birds, however, form the same passages with the same notes,

Compositions for two piping Bullfinches.



at all times, this miſtake of the pitch can never happen in them.

Few ſingers again can continue their own part, whilſt the ſame paſſages are ſung by another in a different key; or if the ſame or other paſſages are ſung, ſo as not to coincide with the muſical bar, or time of the firſt ſinger.

As birds however adhere ſo ſtedfaſtly to the ſame preciſe notes in the ſame paſſages, though they never trouble themſelves about what is called *time* in muſic; it follows that a compoſition may be formed for two piping bulfinches, in two parts, ſo as to conſtitute true harmony, though either of the birds may happen to begin, or ſtop, when they pleaſe.

I have therefore procured ſuch an ingenious compoſition, by a very able muſician *, which I ſend herewith; and it need ſcarcely be obſerved, that there cannot poſſibly be much variety in the part of the ſecond bulfinch. [See TAB. XI.]

Though ſeveral birds have great muſical powers, yet they ſeem to have no delicacy of ſenſations, as the human ſinger hath; and therefore the very beſt of them cannot be taught to exceed the inſipidity of the upper part of the flute ſtop of an organ †, which hath not the modern improvement of a *ſwell*.

* Mr. Zeidler, who plays the violincello at Covent Garden theatre.

† Lord Bacon mentions, that in the inſtrument called a *regall* (which was a ſpecies of portable organ), there was a *nightingale* ſtop, in which water was made uſe of to produce the ſtronger imitation of this bird's tone. See Cent. ii. exper. 172. Though this inſtrument, as well as its *Nightingale* ſtop, is now diſuſed, I have procured an organ pipe to be immerſed partly in water, which, when blown into, hath produced a tone very ſimilar to that of birds.

They

They are alſo eaſily impoſed upon by that moſt imperfect of all inſtruments, a *bird-call*, which they often miſtake for the notes of their own ſpecies.

I have before obſerved, that perhaps no bird may be ſaid to ſing which is larger than a blackbird, though many of them are taught to ſpeak: the ſmaller birds, however, have this power of imitation; though perhaps the larger ones have not organs which may enable them, on the other hand, to ſing.

We have the following inſtances of birds being taught to ſpeak, in the time of the Greeks and Romans, upon which we never try the ſame experiment. Moſchus addreſſes nightingales and ſwallows which were thus inſtructed:

> *Αδονιδες, πασαι τε χελιδονες, ας ποκ᾽ εἑερπεν,*
> *Ας λαλειν εδιδασκε.*
>
> Moſchi Idyl. iii.

Pliny mentions both a cock, thruſh, and nightingales, which articulated *:

"Habebant & Cæſares juvenes turdum †, item
" *luſcinias* Græco atque Latino ſermone dociles, præ-
" terea meditantes in diem, & aſſidue nova lo-
" quentes longiore etiam contextu."

Statius alſo takes notice of ſome birds ſpeaking, which we never attempt to teach in this manner:

* Lib. x. c. 21 & 42.

† Ibid. The other turdus belonged to the Empreſs Agrippina.

 " Huc

" Huc doctæ ſtipentur aves, queis nobile fandi
" Jus natura dedit, plangat Phœbeius ales,
" Auditaſque memor penitus demittere voces
" Sturnus, & Aonio verſæ certamine picæ ;
" Quique refert jungens iterata vocabula perdix,
" Et quæ Biſtonio queritur ſoror orba cubili *."
Stat. Sylv. lib. ii. ecl. 4.

* Amongſt the five birds mentioned in theſe lines of Statius, there are four which are never taught to ſpeak at preſent, viz. the cock, the nightingale, the common, and the *red legged partridge*.

As I ſuppoſe, however, that *perdix* ſignifies this laſt bird, and not the common partridge (as it is always tranſlated), it is proper I ſhould here give my reaſons why I diſſent from others, as alſo why I conceive that *ſturnus*, in this paſſage, is not a *ſtarling*, but the common partridge.

None of the ancients have deſcribed the plumage of the *perdix*; but Ariſtotle, Ovid, and Pliny, inform us of what materials the neſt of this bird is compoſed, as well as where it is placed.

Ariſtotle ſays, that the neſt is *fortified with wood* [a] ; and in another chapter [b], with *thorns and wood*; neither of which are uſed by the common partridge, which often builds in a country where they cannot be procured.

On the contrary, M. de Buffon informs us, that the red legged partridge, " ſe tiennent ſur les montagnes qui produiſent " beaucoup de bruyeres, & de broſſailles [c].

[a] Επηλυγαζομεναι υλην. Lib. v. c. 1. Which Stephens renders *making a covering of wood*.

[b] Lib. ix. c. 8. The common partridge, however, makes its neſt with hay and ſtraw.

[c] Orn. T. II. p. 433.

Silius Italicus alſo deſcribes the *pernix* as being found in the ſame ſort of country,

" Ceu *pernix*, quam denſa vagis latratibus implet
" Venator *dumeta* Lacon." Lib. iii. l. 29.

As

As we find, from theſe citations, that ſo many different ſorts of birds have learned to ſpeak, and as I have ſhewn that a ſparrow may be taught to

Ovid, therefore, ſpeaking of the *perdix*, ſays,

"—— ponitque in ſepibus ova [d],"

where the common partridge is ſeldom known to build.

Pliny again informs us, "perdices ſpinâ & frutice ſic muniunt receptaculum, ut contra feras abunde valentur [e]", as alſo in the 52d chapter of his tenth book, that the *perdix* lays white eggs, which is not true of the common partridge.

But there are not wanting other proofs of the conjecture I have here made.

Ariſtotle, ſpeaking of this ſame bird, ſays, Των μεν περδικων, οι 'μεν κακκαβιζουσιν, οι δε τριζουσι [f].

Now, the word κακκαβιζουσι is clearly formed from the *call* of the bird alluded to, which does not at all reſemble that of the common partridge.

Thus alſo the author of the Elegy on the Nightingale, who is ſuppoſed by ſome to be Ovid, hath the following line:

"*Caccabat* hinc perdix, hinc gratitat improbus anſer."

ſo that the call of the bird muſt have had ſomething very particular, and have anſwered nearly, to the words κακκαβιζει and *caccabat*.

I find, indeed, that M. de Buffon contends [g] that the περδιξ of Ariſtotle does not mean the common partridge, but the bartavel, with regard to which, I ſhall not enter into any diſcuſſion, but only obſerve, that moſt of his references are inaccurate, and that he entirely miſtakes the materials of which the neſt is compoſed, according to Ariſtotle's ſixth book, and firſt chapter.

[d] Ovid. Met. lib. viii. l. 258. I ſhall alſo refer to l. 237, of the ſame book:

"Garrula ramoſâ proſpexit ab ilice perdix:"

as it is well known that the common partridge never perches upon a tree.

[e] Lib. x. c. 23. [f] Lib. iv. c. 9. [g] Orn. T. II. p. 422.

ſing

fing the linnet's note, I fcarcely know what fpecies to fix upon, that may be confidered as incapable of fuch imitations; for it is very clear, from feveral experiments before ftated, that the utmoft endeavours will not be wanting in the bird, if he is endowed with the proper organs.

It can therefore only be fettled by educating a bird, under proper circumftances, whether he is thus qualified or not; for if one was only to determine this point by conjecture, one fhould fuppofe

If, indeed, M. de Buffon had not been thus inaccurate, the grafs and leaves, of which he fays it is made, would prove that Ariftotle fpeaks of the common partridge, contrary to what he himfelf fuppofes.

But the ftrongeft proof that *perdix* fignifies the red legged partridge is, that the Italians to this day call this bird *pernice* [h], and the common fort *ftarna* [i].

This alfo now brings me to the proofs, of *fturnus* in this paffage of Statius fignifying the *common partridge, and not the ftarling*, which I muft admit are not fo ftrong as with regard to the import of the word *perdix*. If my arguments are not therefore fo convincing on this head, the number of birds taught to fpeak by the Romans, and not by us, muft be reduced to three, as the ftarling is frequently learned to talk in the prefent times.

As I cannot argue from the defcription of the habits of the *fturnus*, or the materials of its neft, as in the former inftance, I muft reft my conjecture (fuch as it is) on the two birds, almoft following each other in thefe lines of Statius; on the common partridge being called *ftarna* to this day by the Italians, and upon the Romans having had otherwife no name for our partridge (which is a very common bird in Italy), if *fturnus* is fuppofed to fignify only a *ftarling*.

[h] I cannot therefore but think that *pernix*, according to the reading in the paffage of Silius Italicus, which I have before cited, is the true Latin name for this bird, and that it was called *perdix* by thofe only who underftood Greek.

[i] See Olina.

 that

that a ſparrow would not imitate the ſong of the linnet, nor that a nightingale or partridge could be taught to ſpeak.

And here it may not be improper to explain what I mean by birds learning to imitate the notes of others, or the human ſpeech.

If the birds differ little in ſhape or ſize (particularly of the beak *), the imitation is commonly ſo ſtrong, that

* It ſeems very obvious why the form and ſize of the beak may be material; but I have alſo obſerved, that the colour of a bird's bill changes, when in or out of ſong; and I am informed that a cock ſeldom crows much, but when his comb is red.

When moſt of the finch tribe are coming into ſong, there is ſuch a gradual change in the colour of their bill; thus, thoſe of the chaffinch and linnet are then of a very deep blue, which fades away again, when the bird ceaſes to be in ſong.

This particular ſhould be attended to by the ornithologiſt, in his deſcription; becauſe otherwiſe he ſuppoſes the colour of the bill to be permanent, which is by no means ſo.

This alteration, however, rather ſeems to be the ſymptom than the cauſe of a bird's coming into ſong, or otherwiſe, and I have never attended to this circumſtance in the ſoft billed birds ſufficiently, to ſay whether it holds alſo with regard to them.

A very intelligent bird-catcher, however, was able to prognoſticate, for three winters together, when a nightingale, which I kept ſo long, was coming into ſong (though there was no change in the colour of the bill), by the dung's being intermixed with large bloody ſpots, which before was only of a dead white.

This ſame bird-catcher was alſo very ſucceſsful in his preſcriptions for ſick birds, with regard to the ingredients of which he was indeed very myſterious.

He ſaid, that as he could not feel their pulſe, the circumſtances which he chiefly attended to were their weight, as well as both the conſiſtence and colour of their dung.

He always frankly ſaid what he expected from his preſcriptions, and that if ſuch and ſuch changes did not ſoon take place,

« Mirè

" Mirè ſagaces falleret hoſpites
" Diſcrimen obſcurum." Horat.

for, in ſuch inſtances, the paſſages are not only the ſame, but the tone.

Such was the event of the experiment I have before mentioned of the linnet educated under a vengolina.

In my experiment, however, of teaching the ſparrow the notes of the linnet, though the ſcholar imitated the paſſages of its maſter, yet the tone of the ſparrow had by no means the mellowneſs of the original.

The imitation might therefore be, in ſome meaſure, compared to the ſinging of an opera ſong by a black-guard, when, though the notes may be preciſely the ſame, yet the manner and tone would differ very much.

Thus alſo the linnet, which I heard repeat the words *pretty boy*, did not articulate like a *parrot* or a *mino*, though, at the ſame time, the words might be clearly diſtinguiſhed.

The education I have therefore been ſpeaking of will not give new organs of voice to a bird, and the inſtrument itſelf will not vary, though the notes or paſſages may be altered almoſt at pleaſure.

I tried once an experiment, which might indeed have poſſibly made ſome alteration in the tone of a bird, from what it might have been when the animal

the caſe was deſperate. He frequently alſo refuſed to preſcribe, if the bird felt too light in the hand, or he thought that there was not ſufficient time to bring about an alteration in the dung.

was at its full growth, by procuring an operator who caponiſed a young blackbird of about ſix weeks old; as it died, however, ſoon afterwards, and I have never repeated the experiment, I can only conjecture with regard to what might have been the conſequences of it.

Both Pliny * and the London poulterers agree that a capon does not crow, which I ſhould conceive to ariſe from the muſcles of the larynx never acquiring the proper degree of ſtrength, which ſeems to be requiſite to the ſinging of a bird, from Mr. Hunter's diſſections.

But it will perhaps be aſked, why this operation ſhould not improve the notes of a neſtling, as much as it is ſuppoſed to contribute to the greater perfection of the human voice.

To this I anſwer, that caſtration by no means inſures any ſuch conſequence; for the voices of much the greater part of Italian eunuchs are ſo indifferent, that they have no means of procuring a livelihood but by copying muſic, and this is one of the reaſons why ſo few compoſitions are publiſhed in Italy, as it would ſtarve this refuſe of ſociety.

But it may be ſaid, that there hath been a Farinelli and a Manzoli, whoſe voices were ſo diſtinguiſhedly ſuperior.

To this I again anſwer, that the catalogue of ſuch names would be a very ſhort one; and that we attribute thoſe effects to caſtration, which ſhould rather be aſcribed to the education of theſe ſingers.

* Lib. x. c. 21.

Caſtration

Caſtration commonly leaves the human voice at the ſame pitch as when the operation is performed; but the eunuch, from that time, is educated with a view only to his future appearance on the opera ſtage; he therefore manages his voice to greater advantage, than thoſe who have not ſo early and conſtant inſtruction.

Conſidering the ſize of many ſinging birds, it is rather amazing at what a diſtance their notes may be heard.

I think I may venture to ſay, that a nightingale may be very clearly diſtinguiſhed at more than half a mile *, if the evening is calm. I have alſo obſerved the breath of a robin (which exerted itſelf) ſo condenſed in a froſty morning, as to be very viſible.

To make the compariſon, however, with accuracy, between the loudneſs of a bird's and the human voice, a perſon ſhould be ſent to the ſpot from whence the bird is heard; I ſhould rather conceive that, upon ſuch trial, the nightingale would be diſtinguiſhed further than the man.

It muſt have ſtruck every one, that, in paſſing under a houſe where the windows are ſhut, the ſinging of a bird is eaſily heard, when, at the ſame time, a converſation cannot be ſo, though an animated one.

Moſt people, who have not attended to the notes of birds, ſuppoſe that thoſe of every ſpecies ſing

* Monſ. de Buffon ſays, that the quadruped which he terms the *huarine*, may be heard at the diſtance of a league. Ornith. Tom. I.

exactly

exactly the ſame notes and paſſages, which is by no means true, though it is admitted that there is a general reſemblance.

Thus the London bird-catchers prefer the ſong of the Kentiſh goldfinches, but Eſſex chaffinches; and when they ſell the bird to thoſe who can thus diſtinguiſh, inform the buyer that it hath ſuch a note, which is very well underſtood between them *.

Some of the nightingale fanciers alſo prefer a Surry bird to thoſe of Middleſex †.

Theſe differences in the ſong of birds of the ſame ſpecies cannot perhaps be compared to any thing more appoſite, than the varieties of provincial dialects.

The nightingale ſeems to have been fixed upon, almoſt univerſally, as the moſt capital of ſinging birds, which ſuperiority it certainly may boldly challenge: one reaſon, however, of this bird's being

* Theſe are the names which they give to ſome of the nightingale's notes: *Sweet*, *Sweet jug*, *Jug ſweet*, *Water bubble*, *Pipe rattle*, *Bell pipe*, *Scroty*, *Skeg*, *Skeg*, *ſkeg*, *Swat ſwat ſwaty*, *Whitlow whitlow whitlow*, from ſome diſtant affinity to ſuch words.

† Mr. Henſhaw informs us, that nightingales in Denmark are not heard till May, and that their notes are not ſo ſweet or various as with us. Dr. Birch's Hiſtory of the Royal Society, Vol. III. p. 189. Whilſt Mr. Fletcher (who was miniſter from Q. Elizabeth to Ruſſia) ſays, that the nightingales in that part of the world have a finer note than ours. See Fletcher's Life, in the Biographia Britannica.

I never could believe what is commonly aſſerted, that the Czar Peter was at a conſiderable expence to introduce ſinging birds near Peterſburgh; becauſe it appears, by the *Fauna Suecica*, that they have in thoſe latitudes moſt of the ſame birds with thoſe of England.

more

more attended to than others is, that it ſings in the night *.

Hence Shakeſpeare ſays,

" The nightingale, if ſhe ſhould ſing by day,
" When every gooſe is cackling, would be thought
" No better a muſician than the wren."

The ſong of this bird hath been deſcribed, and expatiated upon, by ſeveral writers, particularly Pliny and Strada.

As I muſt own, however, that I cannot affix any preciſe ideas to either of theſe celebrated deſcriptions, and as I once kept a very fine bird of this ſort for three years, with very particular attention to its ſong; I ſhall endeavour to do it the beſt juſtice I am capable of.

In the firſt place, its tone is infinitely more mellow than that of any other bird, though, at the ſame time, by a proper exertion of its muſical powers, it can be exceſſively brilliant.

When this bird *ſang its ſong round*, in its whole compaſs, I have obſerved ſixteen different beginnings and cloſes, at the ſame time that the intermediate notes were commonly varied in their ſucceſſion with ſuch judgment, as to produce a moſt pleaſing variety.

The bird which approaches neareſt to the excellence of the nightingale, in this reſpect, is the ſkylark; but then the tone is infinitely inferior in point of mellowneſs: moſt other ſinging birds have not above four or five changes.

* The woodlark and reedſparrow ſing likewiſe in the night; and from hence, in the neighbourhood of Shrewſbury, the latter hath obtained the name of the willow-nightingale. Nightingales, however, and theſe two other birds, ſing alſo in the day, but are not then diſtinguiſhed in the general concert.

The next point of superiority in a nightingale is its continuance of song, without a pause, which I have observed sometimes not to be less than twenty seconds. Whenever respiration, however, became necessary, it was taken with as much judgment as by an opera singer.

The skylark again, in this particular, is only second to the nightingale *.

* I shall here insert a table, by which the comparative merit of the British singing birds may be examined, the idea of which I have borrowed from Monf. de Piles, in his *Cours de Peinture par Principes*. I shall not be surprized, however, if, as he suggests, many may disagree with me about particular birds, as he supposes they will do with him, concerning the merits of painters.

As I have five columns instead of the four which M. de Piles uses, I make 20 the point of absolute perfection, instead of 16, which is his standard.

	Mellowness of tone.	Sprightly notes.	Plaintive notes.	Compass.	Execution.
Nightingale	19	14	19	19	19
Skylark	4	19	4	18	18
Woodlark	18	4	17	12	8
Titlark	12	12	12	12	12
Linnet	12	16	12	16	18
Goldfinch	4	19	4	12	12
Chaffinch	4	12	4	8	8
Greenfinch	4	4	4	4	6
Hedge-sparrow . . .	6	0	6	4	4
Aberdavine (or Siskin) .	2	4	0	4	4
Redpoll	0	4	0	4	4
Thrush	4	4	4	4	4
Blackbird	4	4	0	2	2
Robin	6	16	12	12	12
Wren	0	12	0	4	4
Reed-sparrow	0	4	0	2	2
Black-cap, or the Norfolk Mock nightingale [k] .	14	12	12	14	14

[k] Brit. Zool. p. 262.

And

And here I muſt again repeat, that what I deſcribe is from a caged nightingale, becauſe thoſe which we hear in the ſpring are ſo rank, that they ſeldom ſing any thing but ſhort and loud jerks, which conſequently cannot be compared to the notes of a caged bird, as the inſtrument is overſtrained.

I muſt alſo here obſerve, that my Nightingale was a very capital bird; for ſome of them are ſo vaſtly inferior, that the bird-fanciers will not keep them, branding them with the name of Frenchmen *.

But it is not only in tone and variety that the Nightingale excells; the bird alſo ſings (if I may ſo expreſs myſelf) with ſuperior judgement and taſte.

I have therefore commonly obſerved, that my Nightingale began ſoftly like the ancient orators;

I have made no mention of the bulfinch in this table, which is commonly conſidered as a ſinging bird; becauſe its wild note, without inſtruction, is a moſt jarring and diſagreeable noiſe.

I have likewiſe omitted [l] the redſtart (which is called by the French le Roſſignol de Muraille), as I am not ſufficiently acquainted with its ſong, though it is admired by many; I ſhould rather conceive, however, with Zinanni, that there is no very extraordinary merit in the notes.

The London bird-catchers alſo ſell ſometimes the yellow hammer, twite and brambling [m] as ſinging birds; but none of theſe will come within my definition of what may be deemed ſo.

* One ſhould ſuppoſe from this, that the nightingale-catchers had heard much of the French muſic; which is poſſibly the caſe, as ſome of them live in Spittal-fields.

[l] Il culo ranzo é un ucello, (per quanto dicono) molto canoro, ma io tale non lo ſtimo. Delle uova é del nidi, p. 53.

[m] They call this bird a kate.

 reſerving

reſerving its breath to ſwell certain notes, which by this means had a moſt aſtoniſhing effect, and which eludes all verbal deſcription.

I have indeed taken down certain paſſages which may be reduced to our muſical intervals; but though by theſe means one may form an idea of ſome of the notes uſed, yet it is impoſſible to give their comparative durations in point of muſical tune, upon which the whole effect muſt depend.

I once procured a very capital player on the flute to execute the notes which Kircher hath engraved in his *Muſurgia*, as being uſed by the nightingale; when, from want of not being able to ſettle their comparative duration, it was impoſſible to obſerve any traces almoſt of the nightingale's ſong.

It may not be improper here to conſider, whether the nightingale may not have a very formidable competitor in the American mocking-bird*; though almoſt all travellers agree, that the concert in the European woods is ſuperior to that of the other parts of the globe †.

As birds are now annually imported in great numbers from Aſia, Africa, and America, I have frequently attended to their notes, both ſingly and in concert, which are certainly not to be compared to thoſe of Europe.

Thomſon, the poet, (whoſe obſervations in natural hiſtory are much to be depended upon) makes

* Turdus Americanus minor canorus. Ray's Syn. It is called by the Indians Contlatolli; which is ſaid to ſignify four hundred tongues. See alſo Cateſby.

† See Rochefort's Hiſt des Antelles, T. I. p. 366.—Ph. Tr. Abr. Vo. III. p. 563.—and Cateſby.

this

this ſuperiority in the European birds to be a ſort of compenſation for their great inferiority in point of gaudy plumage. Our goldfinch, however, joins to a very brilliant and pleaſing ſong, a moſt beautiful variety of colours in its feathers*.

It muſt be admitted, that foreign birds, when brought to Europe, are often heard to a great diſadvantage; as many of them, from their great tameneſs, have certainly been brought up by hand, the conſequence of which I have already ſtated from ſeveral experiments. The ſoft-billed birds alſo cannot be well brought over, as the *ſuccedaneum* for inſects (their common food) is freſh meat, and particularly the hearts of animals.

I have happened, however, to hear the American mocking-bird in great perfection at Meſſ. Vogle's and Scott's, in Love-lane, Eaſtcheap.

This bird is believed to be ſtill living, and hath been in England theſe ſix years. During the ſpace of a minute, he imitated the woodlark, chaffinch, blackbird, thruſh, and ſparrow. I was told alſo, that he would bark like a dog; ſo that the bird ſeems to have no choice in his imitations, though his pipe comes neareſt to our nightingale of any bird I have yet met with.

With regard to the original notes, however, of this bird, we are ſtill at a loſs; as this can only be

* I cannot but think, that there would be a demand for theſe birds in China, as the inhabitants are very ſedentary, and bird cages are commonly repreſented as hanging in their rooms. I have been informed, by a Tyroleze, that his beſt market for Canary birds was Conſtantinople.

known by thoſe who are accurately acquainted with the ſong of the other American birds.

Kalm indeed informs us, that the natural ſong is excellent *; but this traveller ſeems not to have been long enough in America to have diſtinguiſhed what were the genuine notes: with us, mimics do not often ſucceed but in imitations.

I have little doubt, however, but that this bird would be fully equal to the ſong of the Nightingale in its whole compaſs; but then, from the attention which the *mocker* pays to any other ſort of diſagreeable noiſes, theſe capital notes would be always debaſed by a bad mixture.

We have one mocking bird in England, which is the ſkylark; as, contrary to a general obſervation I have before made, this bird will catch the note of any other which hangs near it; even after the ſkylark note is *fixed*. For this reaſon, the bird-fanciers often place the ſkylark next one which hath not been long caught, in order, as they term it, to keep the caged ſkylark *honeſt*.

The queſtion, indeed, may be aſked, why the wild ſkylark, with theſe powers of imitation, ever adheres to the parental note; but it muſt be recollected, that a bird when at liberty is for ever ſhifting its place, and conſequently does not hear the ſame notes eternally repeated, as when it hangs in a cage near another. In a wild ſtate therefore the ſkylark adheres to the parental notes; as the parent cock attends the young ones, and is heard by them for ſo conſiderable a time.

* Vol. I. p. 219.

I am

I am aware alſo, that it may be aſked, how birds originally came by the notes which are peculiar to each ſpecies. My anſwer, however, to this is, that the origin of the notes of birds, together with its gradual progreſs, is as difficult to be traced, as that of the different languages in nations.

The loſs of the parent-cock at the critical time for inſtruction hath undoubtedly produced thoſe varieties, which I have before obſerved are in the ſong of each ſpecies; becauſe then the neſtling hath either attended to the ſong of ſome other birds; or perhaps invented ſome new notes of its own, which are afterwards perpetuated from generation to generation, till ſimilar accidents produce other alterations. The organs of ſome birds alſo are probably ſo defective, that they cannot imitate properly the parental note, as ſome men can never articulate as they ſhould do. Such defects in the parent bird muſt again occaſion varieties, becauſe theſe defects will be continued to their deſcendants, who (as I before have proved) will only attend to the parental ſong. Some of theſe deſcendants alſo may have imperfect organs; which will again multiply varieties in the ſong.

The truth is, as I before obſerved, that ſcarcely any two birds of the ſame ſpecies have exactly the ſame notes, if they are accurately attended to, though there is a general reſemblance.

Thus moſt people ſee no difference between one ſheep and another, when a large flock is before them. The ſhepherd, however, knows each of them, and can ſwear to them, if they are loſt; as can the Lincolnſhire goſherd to each gooſe.

A

As I now draw towards a conclusion of both my experiments and observations on the singing of birds; it may be possibly asked, what use results either from the trouble or expence which they have cost me; both of which I admit to have been considerable.

I will readily own, that no very important advantages can be derived from them; and yet I shall not decline suggesting what little profit they may possibly be of, though at best they should rather be considered as what Lord Bacon terms, *experiments of light, than of fruit.*

In the first place, there is no better method of investigating the human faculties, than by a comparison with those of animals; provided we make it without a most ungrateful wish of lowering ourselves, in that distinguished situation in which we are placed.

Thus we are referred to the ant for an example of industry and foresight, because it provides a magazine of food for the winter, when this animal is in a state of torpidity during that season; nor are we less willing to suppose the song of birds to be superior to our own musical powers.

The notes of many birds are certainly very pleasing, but can by no means stand in competition either with the human voice or our worst musical instruments; not only from want of the striking effects of harmony in many excellent compositions; but because, even when compared to our simple melody, expression is wanting*, without which music is so languid and inanimate.

* The nightingale, indeed, is perhaps an exception to this general observation.

But

But to return to the uſes (ſuch as they are) which may ariſe from attending to the ſong of birds, or from the experiments which I have given an account of.

The firſt of theſe is too much neglected by the naturaliſt; for, if the bird is not caught, the only means often by which either the ſex or the ſpecies can be determined is the ſong. For example, if Monſ. Adanſon had informed us whether the European ſwallows, which he conceived were to be ſeen during the winter at Senegal, had the ſame notes with thoſe of Europe, it would have been going one ſtep further in proof of the facts which he and others ſo much rely upon.

Theſe experiments, however, may be ſaid to be uſeful to all thoſe who happen to be pleaſed with ſinging birds; becauſe it is clear, that, by educating a bird under ſeveral ſorts, we may often make ſuch a mixture, as to improve the notes which they would have learned in a wild ſtate.

It reſults alſo from the experiment of the linnet being educated under the Vengolina, that we may introduce the notes of Aſia, Africa, and America, into our own woods; becauſe, if that linnet had been ſet at liberty*, the neſtlings of the next ſeaſon would have adhered to the Vengolina ſong, who would again tranſmit it to their deſcendants.

* I know well, that it is commonly ſuppoſed, if you ſet a caged bird at liberty, it will neither be able to feed itſelf, nor otherwiſe live long, on account of its being perſecuted by the wild ones. There is no foundation, however, for this notion; and I take it to ariſe from an excuſe for continuing to keep theſe birds in confinement.

But

But we may not only improve the notes of birds by a happy mixture, or introduce thofe which were never before heard in Great-Britain; as we may alfo improve the inftrument with which the paffages are executed.

If, for example, any bird-fancier is particularly fond of what is called the fong of the Canary bird, which however muft be admitted to be inferior in tone to the linnet, it would anfwer well to any fuch perfon, if a neftling linnet was brought up under a Canary bird, becaufe the notes would be the fame, but the inftrument which executes them would be improved.

We learn alfo, from thefe experiments, that nothing is to be expected from a neftling brought up by hand, if he does not receive the proper inftruction from the parent cock: much trouble and fome coft is therefore thrown away by many perfons in endeavouring to rear neftling nightingales, which, when they are brought up and fed at a very confiderable expence, have no fong which is worth attending to.

If a woodlark, or fkylark, was educated, however, under a nightingale, it follows that this charge (which amounts to a fhilling per week *) might be in a great meafure faved, as well as the trouble of chopping frefh meat every day.

A nightingale, again, when kept in a cage, does not live often more than a year or two; nor does he fing more than three or four months; whereas the

* Olina fpeaks of a pafte which is ufed in Italy for nightingales; but I cannot find that it ever anfwers with us; perhaps, they bring their nightingales up by hand, and fo accuftom them from their earlieft infancy to fuch food.

ſcholar pitched upon may not only be more vivacious, but will continue in ſong nine months out of the twelve.

I fear, however, that I have already dwelt too long upon theſe very minute and trifling advantages which may reſult from my experiments and obſervations; I ſhall therefore no longer defer ſubſcribing myſelf,

Dear Sir,

Your moſt faithful,

Humble Servant,

Daines Barrington.

Received January 8, 1773.

XXXII. *An Account of the* Tokay *and other Wines of* Hungary, *by* Sylvefter Douglafs, *Efquire: Communicated by* Edward Poore, *Efq*, *F. R. S.*

Read June 10, 1773.

AS the growth, quantity, and value of the Tokay wine are little known in this country, and the popular notions concerning them are in general erroneous, it may be a matter both of fome curiofity and ufe, to throw together what I was able to collect on this fubject, in the country where it grows, as well as from the chief proprietors of it, both in Hungary and at Vienna. I fhall fubjoin a brief account of the other moft remarkable Hungarian wines.

Geography. The town, or rather village, of Tokay, from whence this celebrated wine derives its name, ftands at the foot, and to the eaft of a high hill, clofe by the conflux of the river Bodrug, with the Theis or Tibifcus. In the Norimberg map of Hungary, it is erroneoufly placed between thefe rivers, for it is on the weft fide of both. The inhabitants are chiefly either Hungarians of the Proteftant religion, or Greeks, who came originally from Turkey, but have

have been long ſettled here for the purpoſe of carrying on the wine trade. During the two laſt centuries, this country being almoſt the conſtant theatre of war, there was a citadel near the town, but now there is not the ſmalleſt veſtige of it remaining.

The hills on which the wine grows lie all to the weſt of the river Bodrog and beginning cloſe by the town of Tokay, extend weſtward and northward from thence, and occupy a ſpace of perhaps ten Engliſh miles ſquare; but they are interrupted and interſperſed with a great many extenſive plains, and ſeveral villages, ſuch as Talia, Madá, Tarczal, Szombor, Benye, and Toleſwa. Near ſome of theſe, particularly Talia and Tarczal, the wine is better than what grows on the hill of Tokay, but it all goes under the ſame general name.

The vineyards extend beyond the 48th degree of northern latitude. Latitude.

The ſoil, on all the hills where the wine grows, is a yellow clayiſh earth, extremely deep, and there are interſperſed through it large looſe ſtones, which, as I was told, are limeſtone; but I had not an opportunity of examining them. Soil.

As the hills do not run in a regular chain, but are ſcattered among the intervening plains, you meet with all kinds of expoſures upon them, and there is wine on them all, except perhaps where they are turned directly towards the ſouth. Yet the general rule is, that the expoſures moſt inclining to the ſouth, the ſteepeſt declivities, and the higheſt part of thoſe declivities, produce the beſt wine. Theſe circumſtances ſhew the advantage of chooſing your wine on the ground. The late Mr. Wortley Montague Expoſure.

tague was so well convinced of this, that a few years before his death he undertook a journey from England to Tokay, and continued there several months, in order to be sure of having the best and most genuine wine.

Quantity. It is a vulgar error, that the Tokay wine is in so small quantity, as never to be found genuine, unless when given in presents by the court of Vienna. The extent of ground on which it grows is a sufficient proof to the contrary. It is a common desert wine in all the great families at Vienna, and in Hungary, and is very generally drank in Poland and Russia, being used at table in those countries, like Madeira in this.

Proprietors. Another vulgar error is, that all the Tokay wine is the property of the Empress Queen. She is not even the most considerable proprietor, nor of the best wine; so that every year she sells off her own, and purchases from the other proprietors, to supply her own table, and the presents she makes of it. The greatest proprietor is the Prince Trautzon, an old man, at whose death, indeed, his estate will escheat to the crown; but many others of the German and Hungarian nobility have large vineyards at Tokay; most of the gentlemen in the neighbourhood have part of their estates there; the Jesuits college at Ungwar has a considerable share of the best wine; and besides these, there are many of the peasants who have vineyards, which they hold of the Queen, or other lords, by paying a tythe of the annual produce.

Grapes. There is never any red wine made at Tokay, and, as far as I recollect, the grapes are all white. They

They are ſuppoſed to have a particular flavour, which I own I could not perceive, though they were beginning to be ripe when I was there, in the end of Auguſt (1768), and I have often eat of ſuch as have been brought to Vienna.

The vintage is always as late as poſſible. It commonly begins at the feaſt of St. Simon and Jude, October 28, ſometimes as late as St. Martin's, November 11. This is determined by the ſeaſon, for they have the grapes on the vines as long as the weather permits, as the froſts, which from the end of Auguſt are very keen during the nights, are thought to be of great ſervice to the wine. By this means it happens, that when the vintage begins, a great many of the grapes are ſhrivelled, and have, in ſome meaſure, the appearance of dried raiſins. Vintage.

There are four ſorts of wine made from the ſame grapes, which they diſtinguiſh at Tokay by the names of Eſſence, Auſpruch, Maſslaſch, and the common wine. Different ſorts.

The proceſs for making them is as follows.

The half-dried and ſhriveled grapes, being carefully picked out from the others, are put into a perforated veſſel, where they remain as long as any juice runs off by the mere preſſure of their own weight. This is put into ſmall caſks, and is called the Eſſence. 1. The Eſſence.

On the grapes from which the eſſence has run off, is poured the expreſſed juice of the others from which they had been picked, and then they tread them with their feet. The liquor obtained in this manner ſtands to ferment during a day or two, after which it is poured into ſmall caſks, which are kept 2. Auſpruch.

in

in the air for about a month, and afterwards put into the cellars. This is the Aufpruch.

3. Mafslafch. The fame procefs is again repeated, by the addition of more of the common juice to the grapes which have already undergone the two former preffures, only they are now alfo wrung with the hands, and this gives the Mafslafch *.

The fourth kind. The fourth kind is made by taking all the grapes together at firft, and fubmitting them to the greateft preffure. It is chiefly prepared by the peafants, who have not a fufficient quantity of grapes, and cannot afford the time and apparatus neceffary for making the different forts. It is, I believe, entirely confumed in the country, and forms the common *Vin du pais.*

The Effence is thick, and never perfectly clear, very fweet and lufcious. It is chiefly ufed to mix with the other kinds, and when joined to the Mafflafch, forms a wine equally good with the Aufpruch, and often fold for it.

The Aufpruch is the wine commonly exported, and what is known in foreign countries under the name of Tokay. The following are the beft rules for judging of it; though in this, and all fimilar cafes, it requires experience to be able to put fuch rules in practice.

1. The colour fhould neither be reddifh (which it often is) nor very pale, but a light filver.

2. In trying it, you fhould not fwallow it immediately, but only wet your palate and the tip of the

* I cannot anfwer for the orthography of this and the foregoing term, having written them by the ear, as they were pronounced.

tongue.

tongue. If it discover any acrimony to the tongue (or bite it), it is not good. The taste ought to be soft and mild.

3. It should, when poured out, form globules in the glass, and have an oily appearance.

4. When genuine, the strongest is always of the best quality.

5. When swallowed, it should have an earthy astringent taste in the mouth, which they call the *Taste of the root.* The Poles, particularly, are fond of this astringency and austerity in their Tokay. There is so great a difference between the Tokay used in Poland and what I drank both at Tokay and Vienna, which, I am sure, was of the best and most genuine kind, that I am inclined to think their wine is composed of the Masslasch, which, by the severe pressure it suffers, must carry with it much of the astringent quality which, in all grapes, resides in the skin, and a smaller proportion than usual of the essence. But this is mere conjecture.

Besides the qualities already mentioned, all Tokay wine has an aromatic taste; so peculiar, that nobody, who has ever drank it genuine, can confound it with any other species of wine. The only species that bears a resemblance to it grows, in a very small quantity, in the Venetian Friule, and is only to be met with in private families at Venice, where, in the dialect of the place, it is called *Vin piccolit.*

The Tokay wine, both the Essence and Auspruch, Age.
keeps to any age, and improves by time. I have drank of the latter at Vienna, which had been in the same cellar since the year 1686. It is never good till it is about three years old.

All

Meaſure. All the ſorts are generally kept in ſmall caſks, called antheils, which *legally* hold 80 Hungarian mediæ, a meaſure containing about two thirds of an Engliſh quart. When you buy it of the gentlemen who are proprietors, you have commonly more than the legal quantity in the antheil; if from the Greek merchants, always leſs.

Price. The particular year, or vintage, and the age, vary the price of this as of all other wines.

The medium price of the antheil of Eſſence is between 60 and 70 ducats. It is ſometimes ſold on the ſpot for more than 100. Prince Radzivil paid 300 ducats for two antheils about four years ago. When the price is 60 ducats, and the antheil large meaſure, that is, about 90 mediæ, it is exactly a ducat the Engliſh quart.

The price of the Auſpruch is from 26 to about 30 ducats the antheil. This is at the rate of two florins, or near a crown the Engliſh quart. The Empreſs Queen, a little before I was at Tokay, purchaſed ſome of vintage 1754 at 33 ducats, and this was looked upon as an extraordinary price. The variety in the prices of the Eſſence and Auſpruch, accounts for the oppoſite relations of people, who ſay ſometimes that it coſts half a guinea, ſometimes five ſhillings, on the ſpot.

There are people who come every year from Poland, about the time of the vintage, to chooſe their own wine on the ground, and ſee it carefully managed. But it is a falſe opinion of many, that they contract for the wine of ſeveral years forwards: no ſuch thing has ever been practiſed. For theſe laſt twenty years the court of Peterſbourg has had an

agent, who refides conftantly at Tokay, for the purpofe of buying wine. The prefent agent is a major in the Ruffian fervice, and formerly was a major-general. He commonly purchafes every year from 40 to 60 antheils of Aufpruch, but never of any other fort.

One of the principal proprietors, who refides at Cafchaw *, told me, that he would engage to deliver the beft old Tokay (i. e. Aufpruch), in bottles, at London, for a ducat a bottle, containing two thirds of a quart, or confiderably cheaper in cafk, and would put his own feal on it, and infure it.

It is much the beft way to tranfport it in cafks; for when it is on the feas, it ferments three times every feafon, and refines itfelf by thefe repeated fermentations. When in bottles, there muft be an empty fpace left between the wine and the cork, otherwife it would burft the bottle. They put a little oil on the furface, and tie a piece of bladder on the cork. The bottles are always laid on their fides in fand.

I am perfuaded an Englifh merchant, or company of merchants, would find their account in eftablifhing a correfpondence with one of the principal proprietors in the country, or in fending an agent to refide at Tokay, who might watch the opportunity of the good vintages, choofe the beft expofures, and bargain with the proprietors themfelves. They fhould have cellars there to keep the wine to a proper age, and an agent at Warfaw, and another at Dantzic, to receive it. This is the road it muft take. The carriage, as I was informed, from Tokay to the fron-

* The Baron Vefey, prefident of the chamber of Cafchaw.

 tiers

tiers of Poland, coſts the Poliſh merchants two ducats the antheil, and three florins of duty. The duties in Poland amount to about 14 florins the antheil. The carriage down the Viſtula cannot be very expenſive. Every charge included, I am perſuaded the beſt old Tokay might be ſold in London at half a guinea the quart bottle, which is 100 *per cent.* leſs than what the worſt often coſts.

Of the other Hungarian Wines.

There is not, I believe, in Europe any country which produces a greater variety of wines than Hungary. I have been told, at Vienna, of an entertainment given to the Empreſs Queen at Preſburg, by an Hungarian nobleman, where there were above a hundred different ſorts of wine produced, all the growth of that country. Many of them are of an excellent quality, and would undoubtedly be a ſource of great commerce and riches to that kingdom, if it enjoyed a maritime, inſtead of its inland, ſituation.

The moſt valuable white wines after the Tokay, are,

1. *The St. George wine*, which grows near a village of that name, about two German miles north of Preſburg, and in the ſame latitude with Vienna. This wine approaches the neareſt of any Hungarian wine to Tokay. Formerly they uſed to make Auſpruch at St. George; but this was prohibited by the court about ſixteen years ago, it being ſuppoſed that it might hurt the traffic of the Tokay wine. The prohibition ſhews, that the method of preparing the

the Tokay is thought to have a ſhare in its peculiar qualities.

2. *The Edenburg wine*, reſembling the St. George, but inferior in quality and value. Edenburg is a town ſituate about nine German miles north-weſt of Preſburg.

3. *The Carlowitz wine*, ſomething like that of the *Côte rotie* on the banks of the Rhone. Carlowitz is the ſeat of the metropolitan of the Greek church in Hungary. It ſtands on the banks of the Danube, between 45 and 46 degrees of latitude.

The beſt red wines are,

1. *The Buda wine*, which grows in the neighbourhood of the antient capital of the kingdom. This wine is like, and perhaps equal to, Burgundy, and is often ſold for it in Germany. A German * author of the laſt century ſays, that a great quantity of this wine uſed to be ſent to England in the reign of James I. over land by Breſlaw and Hamburg, and that it was the favourite wine both at court and all over England.

2. *The Sexard wine*, a ſtrong deep-coloured wine, not unlike the ſtrong wine of Languedoc, which is ſaid to be ſold at Bordeaux for claret. The Sexard wine on the ſpot coſts about 5 creuzers, or 2½ *d.* a bottle. It belongs to the Abbot of Conſtance, and is chiefly conſumed in Germany. Sexard is on the Danube, between Buda and Eſſeh.

3. *The Erlaw wine*, which is reckoned at Vienna almoſt equal to that of Buda. Erlaw is in Upper

* Hevelius Sileſiograph. 2 vols. art. *Breſlaw.*

Hungary, ſouth-weſt of Tokay, between 47 and 48 degrees of latitude.

4. *The Gros Wardein wine,* a ſtrong bodied wine, and very cheap. It belongs chiefly to the Duke of Modena, whoſe anceſtor got a large eſtate in this country, in grant from the Emperor Leopold, as a reward for his ſervices in the Hungarian wars. Gros Wardein is an old fortreſs near the confines of Tranſylvania, between 46 and 47 degrees of latitude.

XXXIII. *On the Figure and Composition of the Red Particles of the Blood, commonly called the Red Globules. By Mr.* William Hewson, *F. R. S. and Teacher of Anatomy.*

Read June 17, and 24, 1773.

THE red particles of the blood in the human subject have, since the time of Leeuwenhoeck, been so generally allowed to be spherical, that in almost all books of physiology they are denominated red globules. A few authors, however, have at different times doubted whether they were spheres, and amongst the rest Father de la Torré, whose curious observations, together with his glasses, were presented to the Royal Society, Anno 1766. As I flatter myself that I have made some new observations on these particles, I shall do myself the honour of communicating them to the Society.

It is a curious and important fact, that these particles are found so generally through the animal kingdom; that is, they are found in the human species, in all quadrupeds, in all birds, in all amphibious animals, and in all fish, in which animals they are red, and colour the blood. The blood even of insects contains particles similar in shape to those of the blood of more perfect animals, but differing

fering in colour. In water inſects, as lobſters and ſhrimps, theſe particles are white; in ſome land inſects, as the caterpillar and the graſs-hopper they appear of a faint green, when in the veſſels as I am perſuaded from experiments. I have ſeen them in an inſect no bigger than a pin's head, and ſuſpect they exiſt almoſt univerſally through the animal kingdom.

What is ſo generally extended through the creation muſt be of great importance in animal œconomy, and highly deſerving the attention of every enquirer into the works of nature. This ſubject becomes the more intereſting from ſo much of reaſoning in the theory of medicine being built in the properties of thoſe particles.

It is by the microſcope alone that we can diſcover theſe particles; and as ſome dexterity and practice is required in the uſe of that inſtrument, there have not been wanting men of character and ingenuity, who, having been unſucceſsful in their own experiments, have queſtioned the validity of thoſe made more fortunately by others. Some have gone ſo far as to aſſert, that no credit can be given to microſcopes, that they deceive us by repreſenting objects different from what they really are. Theſe aſſertions, though not entirely without foundation, when we ſpeak of one ſort of microſcopes, are very unjuſtly applied to them all. In compound microſcopes, when the object is viewed through two or more glaſſes, if theſe glaſſes be not well adapted to the focus of each other, the figure of the object may be diſtorted; but no ſuch circumſtance takes place, when we view an object through a ſingle lens. All who uſe ſpectacles agree, that the figures of objects appear the ſame through them, as they do

do to the naked eye. And as the ſingle microſcope has, like the ſpectacle, but one lens between the eye and the object, there is no reaſon to ſuppoſe the one can deceive us more than the other. The compound, having a larger field, is more pleaſant than the ſingle microſcope for many purpoſes; but the ſingle ſhould be always preferred by thoſe who wiſh to aſcertain the figures of minute bodies. It was this inſtrument ſupported on a ſcroll, as delineated by Mr. Baker (Microſcope made eaſy, plate II. chap. 3.) that has been uſed in theſe experiments: and almoſt all the obſervations were made with lenſes, as they are prepared by ſome of our more ſkilful workmen in London. One obſervation only was made by means of thoſe globules made of glaſs, which the ingenious Father de la Torré preſented to the Royal Society, and which they were ſo obliging as to lend to me. Of theſe globules but two were fit for uſe, when they came into my hands, viz. that which, according to Father de la Torré, magnifies the diameter of the object 640 times, and that which magnifies 1280 times. The lenſes of the greateſt magnifying power made in London are thoſe of $\frac{1}{50}$th of an inch focus, which, even allowing eight inches to be the focal diſtance of the naked eye, magnifies the diameter of the object only 400 times; a power much inferior to what may be obtained by globules, and particularly by that globule, which according to Father de la Torré magnifies the diameter 1280 times; and this globule I have uſed in ſome of theſe experiments. But our lenſes, though inferior in magnifying power to theſe globules, are much ſuperior in diſtinctneſs; the globules

globules are full of clouds, made by the ſmoke of the lamp uſed in preparing them, and the object can be ſeen only through the tranſparent parts of the globules, which makes it difficult to get a ſatisfactory view of it; this, with the trouble of adapting the objects to the focus of the glaſs, made me prefer our own lenſes for all the experiments mentioned in theſe ſheets, except one; and it is but doing juſtice to the ingenious gentleman abovementioned, to acknowledge, that the greater power of his glaſſes was found in that experiment more than to compenſate for their want of diſtinctneſs.

Theſe particles of the blood, improperly called globules, are in reality flat bodies. Leeuwhenhoeck and others have allowed, that in fiſh and in the *amphibia* they are flat and elliptical; but in the human ſubject and in quadrupeds almoſt all microſcopical obſervers have agreed in their being ſpherical. When we conſider how many ingenious perſons have been employed in examining the blood with the beſt microſcopes, it will appear wonderful, that the figure of theſe particles ſhould have been miſtaken; but our wonder will be leſſened, when we conſider how many obvious things are overlooked, till our attention is very particularly directed to them; and beſides, the blood in the human ſubject and in quadrupeds is ſo full of theſe particles, that it is with great difficulty we can ſee them ſeparate, unleſs we find out a method of diluting the blood. It is to ſuch a diſcovery that I attribute my ſucceſs in this enquiry; for, having examined the blood as it flows from the veſſels of the human body, it appeared a confuſed maſs, notwithſtanding I ſpread

I ſpread it thin on a glaſs, or a piece of talk. It then occurred to me to dilute it, but not with water, for this I knew diſſolved the particles; but with ſerum, in which they remain undiſſolved. By the ſerum I could dilute it to any degree, and therefore could view the particles diſtinct from each other; and in theſe experiments I found that theſe particles of the blood were as flat as a guinea. I likewiſe obſerved that they had a dark ſpot in the middle, which Father de la Torré took for a hole; but, upon a careful examination, I found it was not a perforation, and therefore that they were not annular. I next made experiments by mixing theſe particles with a variety of other fluids, and examined them in many different animals; and the reſult of theſe experiments was, that their ſize is different in different animals, as is ſeen in plate XII, where they are repreſented of the ſize they appeared to my eye, when viewed through a lens of $\frac{1}{23}$ of an inch focus, which, allowing eight inches to be the focal diſtance of the naked eye, magnifies the diameter 184 times.

It may not be improper to obſerve here, that the accurate Leeuwenhoeck not having diluted the human blood, or that of quadrupeds, ſo as to ſee theſe particles ſeparate from each other, was thence not qualified to deſcribe them from his own obſervation, as he has done thoſe of fiſh and of frogs; and, ſuſpecting a round figure was more fit for circulating in our veſſels, was thence led to ſuppoſe theſe particles ſpherical in the human ſubject. But I ſhall hereafter be able to ſhew from his own words, that it is not his obſervations, but

his ſpeculative opinions, or his theory, that differs from what I have diſcovered by theſe experiments.

In plate XII. it appears, that of all the animals which I have examined, the particles are largeſt in the fiſh called a ſkate; next to the ſkate they are largeſt in a frog and a viper, and other animals of this claſs; they are ſomewhat ſmaller in the common fiſh, as the ſalmon, cod, and eel. In birds they are ſmaller than in fiſh; in the human ſubject ſmaller than in birds; and in ſome quadrupeds ſtill ſmaller than in the human ſubject. Leeuwenhoeck, ſpeaking of their ſize, ſays, he is confident the red particles of the blood are no larger in a whale than in the ſmalleſt fiſh *. And others have, ſince his time, ſaid they are of the ſame ſize in all animals; but it is evident, from comparing their ſize, as delineated in the abovementioned plate, that it differs conſiderably, and that they are not largeſt in the largeſt animals; for we find that in an ox they are not ſo large as in a man; and ſo far are they from being larger in the whale than in the ſmall fiſh, it appears probable, from comparing their ſize, as delineated plate XII. N° II. from a porpus, which belongs to the ſame genus as the whale, that they are ſmaller in thoſe animals than in fiſh. Neither is their ſize inverſely, as the ſize of the animal; for they are as large in an ox as in a mouſe. The difference in their ſize therefore depends on ſome other circumſtance than a difference in the ſize of the animal.

* Conſ. Arcan. Nat. p. 220.

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As to their ſhape, I have already mentioned that they are flat in all animals, even in the human ſubject; of which any one may be convinced by repeating the following experiments.

EXPERIMENT I. Take a ſmall quantity of the ſerum of human blood, and ſhake a piece of the craſſamentum in it, till it is coloured a little with the red particles; then with a ſoft hair pencil ſpread a little of it on a piece of thin glaſs, and place this glaſs in the microſcope in ſuch a manner as not to be quite horizontal, but higher at one end than the other, by which means the ſerum will flow from the higher extremity to the lower, and as it flows, ſome of the particles will be found to ſwim on their flat ſides, and will appear to have a dark ſpot in their middle; others will turn over from one ſide to the other as they roll down the glaſs. No perſon who ſees them turn over can doubt of their being flat; he will ſee them in turning have all the phaſes that a flat body has; firſt he will ſee them on one ſide, then riſe gradually upon their edge, and turn over to the other ſide. I have in this way ſhewed their figure to a number of curious perſons, and particularly to many ſtudents of anatomy, who have attended lectures in London within the laſt ſix years.

If, inſtead of ſerum, the particles ſhould be diluted with water containing rather more ſalt than ſerum does; or if, inſtead of human blood, that of an animal with larger particles be uſed: then they will ſometimes be ſeen not only flat, but a little bended, like a crooked piece of money.

Theſe experiments not only prove that the particles of the blood are flat, and not globular; but likewiſe, by proving that they are flat, they ſhew that they are not fluid, as they are commonly believed to be; but, on the contrary, are ſolid; becauſe every fluid ſwimming in another, which is in larger quantity, if it be not ſoluble in that other fluid, becomes globular; this is the caſe where a ſmall quantity of oil is mixed with a larger quantity of water, or if a ſmall quantity of water be mixed with a large one of oil, then the water appears globular. And as theſe particles are not globular, but flat, they muſt be ſolid; a circumſtance that will appear ſtill more evident from future experiments.

It is neceſſary to remark, that in a few minutes after the particles are ſpread out on a glaſs, they run in cluſters, and ſtick to each other, and then they appear confuſed.

When one of theſe particles is attentively examined, while ſeparate from the reſt, it appears, as it lies on its flat ſide, to have a dark ſpot in the middle, and round that dark ſpot it is more tranſparent. This dark ſpot was believed to be a perforation, or the particle was ſuppoſed to be a hollow ring, by the ingenious Father de la Torré. But I find, from a great number of experiments, that the dark ſpot is a ſolid particle contained in a flat veſicle, whoſe middle only it fills, and whoſe edges are hollow, and either empty, or filled with a ſubtile fluid. This will be evident to every one who will carefully make the following experiments.

EXPERI-

Experiment II. Take a drop of the blood of an animal that has large particles, as a frog, a fish, or, what is still better, of a toad; put this blood on a thin piece of glass, as used in the former experiment, and add to it some water, first one drop, then a second, and a third, and so on, gradually increasing the quantity; and in proportion as water is added, the figure of the particles will be changed from a flat to a spherical shape. When much water is added, the vesicle will by degrees become thinner, and more transparent, and will at last be dissolved. When the vesicle has thus assumed a spherical shape, it will roll down the glass stage smoothly, without those phases which it had when turning over whilst it was flat; and as it now rolls in its spherical shape, the solid middle particles can be distinctly seen to fall from side to side in the hollow vesicle like a pea in a bladder. Sometimes, indeed, instead of falling from side to side, the solid middle particle is seen to stick to one part of the vesicle; and in proportion as the vesicle, instead of being flat, assumes a spherical shape, its longest diameter is shortened as might be expected, on the supposition of its being hollow and flat.

After this experiment has been made on the blood of such animals as have large vesicles, it may be made on human blood, where the water will be found to have the same effect; the vesicles will become spherical, the diameters of these spheres will be less than the largest diameter of the vesicle was, in its flat state.

It

It is remarkable that more water is in general required to produce this change on the veſicles of the human blood than on thoſe of frogs, or other amphibious animals; and thoſe of the amphibia require ſtill more than thoſe of fiſh; for the ſubſtance of theſe veſicles being thicker and more coloured in man and in quadrupeds than in the amphibia, is therefore later in being diſſolved in water; and being thinneſt in fiſh, it thence moſt readily diſſolves. Thoſe who are deſirous of repeating theſe experiments had beſt begin with the blood of toads and frogs, whoſe veſicles are large, and remain ſome time without diſſolving in the water (when that is uſed with the above-mentioned precautions); by which means any one accuſtomed to microſcopical experiments may readily be ſatisfied of theſe curious circumſtances.

From the greater thickneſs of the veſicles in the human ſubject, and from their being leſs tranſparent when made ſpherical by the addition of water, and likewiſe from their being ſo much ſmaller than thoſe of fiſh or frogs, it is more difficult to get a ſight of the middle particles rolling from ſide to ſide in the veſicle, which has become round; but with a ſtrong light *, and a deep magnifier, I have diſtinctly ſeen it in the human ſubject, as well as in the frog, toad, or ſkate.

Since water makes theſe particles round, and makes the dark ſpot in their middle diſappear, it is evident the red particles of the human blood are not perforated; but that dark ſpot is owing to ſomething elſe than a hole: and this is likewiſe con-

* Theſe experiments were all made with day-light, in clear weather.

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firmed by obſerving that although the particle does in an obſcure glaſs appear only to have a dark ſpot which might be ſuppoſed to be a hole, yet, with a very tranſparent lens, and a good light, after diluting the blood with ſerum, that middle part can be diſtinctly ſeen to be only of a deeper red than the reſt of the veſicle, and thence appears darker.

In theſe experiments, made by adding water to the blood, the middle particles appear to be leſs eaſily ſoluble in water than the flat veſicle which contains them; ſo that, a little time after the proper quantity of water has been added, the flat veſicles diſappear, leaving their middle particles; which ſeem to be globular, and very ſmall.

That theſe red veſicles of the blood, although flat, are not perforated, is evident likewiſe from a curious appearance which I have repeatedly obſerved in blood that has been kept three days in the ſummer ſeaſon, ſo that it was beginning to putrefy; the veſicles of this blood being diluted with ſerum, and examined with a lens $\frac{1}{50}$ of an inch focus, but more particularly when examined with M. de la Torré's glaſs, which, by his computation, magnifies the diameter 1280 times, were found to have become ſpherical; the diameter of theſe ſpheres was leſs than their largeſt diameter when flat, and their external ſurface was corrugated in ſuch a manner as to make them appear like ſmall mulberries.

I have ſeen the ſame appearance on mixing ſerum that had been kept three days in a warm place, and ſmelt putrid, with freſh-drawn human blood; the veſicle aſſumed this globular and mulberry-like appearance.

In

In theſe experiments on human blood beginning to putrefy, I have likewiſe obſerved ſome of theſe veſicles break into pieces without becoming ſpherical; and I have diſtinctly perceived the black ſpot in the center fiſſured through its middle, another proof that it is not a perforation.

In the blood of an eel, which was beginning to putrefy, I have ſeen the veſicles ſplit and open, and the particle in its center come out of the fiſſure.

As the putrefaction advances, thoſe veſicles which had become rough ſpheres, or like mulberries, and thoſe which had been merely fiſſured, each break down into ſmaller pieces. M. de la Toré ſeems to think they have joints, and break regularly into ſeven parts; and Leeuwenhoeck ſuſpected theſe globules, as he called them, were conſtantly made of ſix leſſer globules. But from obſervations I am convinced there is nothing regular or conſtant in the number of pieces into which they break. I have ſeen them fall into ſix, ſeven, eight, or more pieces, by putrefaction; for putrefaction breaks them down in the manner it deſtroys other animal ſolids.

I need hardly take notice, that the ſmall pieces into which the veſicles break are equally red as the veſicle itſelf. The theory of the red globules being compoſed of ſix ſerous ones compacted together, and the ſerous globules of ſix of lymph, has not the leaſt foundation, and is intirely overthrown by the ſimple experiment of mixing the blood with ſix or with thirty-ſix times its quantity of water; for the water diſſolving the globules ought to reduce them to yellow ſerum, or colourleſs lymph *: but it does

* See Gaubii Pathologia.

2 not;

not; on the contrary, it is coloured red by these particles, even when used in much greater proportion than thirty-six parts of water to one of blood.

These red vesicles of the blood have not only been commonly supposed globular and fluid, but they have with equal injustice been imagined to be oily and more inflammable than the rest of the blood. That they are not oily is evident from their so readily dissolving in water; and that they are not more inflammable than the rest of the blood is manifest by burning them after they are separated them from the rest of the blood, which separation may be effected by shaking the crassamentum in the serum so as to diffuse the particles through it; and then, by pouring off the clear serum, when they have subsided in it. I have separated them in this manner, and compared their inflammability with that of inspissated serum, and of dried coagulable lymph, and have not observed them more inflammable than the serum or the lymph; nor do they melt like oils, as some have suspected, but burn like a piece of horn.

Some authors, who have written on the figure of these vesicles in quadrupeds, and in the human subject, have expatiated on the great advantages of their (supposed) spherical shape, in order for their more easy circulation; as it is probable that no form is preferable to a spherical one for easy motion. But as these vesicles are evidently not spherical but flat in all animals, we must believe that nature has some good purpose to answer by making them of that form.

It has been objected, that, notwithstanding they appear flat out of the body, they may possibly be

globular in the body, while circulating; and it has been ſaid, that it is almoſt inconceivable that ſo many ingenious men ſhould at different times have viewed them through a microſcope, and have concluded them ſpherical, if they be really flat. But, however that may have happened, it is a fact that they are as flat in the body as out of it. Of this I am convinced by having repeatedly obſerved them whilſt circulating in the ſmall veſſels between the toes of a frog, both in the ſolar microſcope, and the more ſimple one abovementioned. I have ſeen them with their ſides parallel like a number of coins laid one againſt another. I have likewiſe in that animal, where they are elliptical, ſeen them move with one end foremoſt, and ſometimes with an edge turned towards the eye. I have moreover ſeen them, when entering a ſmall veſſel, ſtrike upon the angle between it and the larger trunk, and turn over with the ſame variety of phaſes that they have when turning over upon a piece of glaſs.

Upon this occaſion I may remark, that it has been ſaid by ſome microſcopical obſervers, that in paſſing through very ſmall veſſels they ſeem to alter their ſhapes, and to be lengthened. This concluſion, I ſuſpect, has taken its riſe from the obſerver having ſeen them with their edge turned towards his eye; in which caſe they would appear long and ſmall, as if lengthened by compreſſion, eſpecially to one who ſets out with the notion of their being globular. I have ſeen them in blood veſſels, which would admit only ſingle veſicles, move with difficulty, as if ſtreightened for room; but never ſaw them altered in their ſhape by the action of the veſſels.

If then they really be not globular but flat, and if water so readily alter their shape, whence is it that the serum has the property of preserving them in that form which seems so necessary, because it is so general through the animal creation?

It is principally by the salts of the serum that this effect is produced, as is proved by adding a small quantity of any neutral salt to water, when the water is no longer capable of dissolving those particles, nor does it alter their shape when the salt is used in a certain proportion.

Experiment III. If a saturated solution of any of the common neutral salts be mixed with fresh blood, and the globules (as they have been called, but which for the future I shall call flat vesicles) be then examined in a microscope; the salt will be found to have contracted or shriveled the vesicles, so that they appear quite solid, the vesicular substance being closely applied all round the central piece. In proportion as the solution of salt is diluted with water, it has less effect; and, when diluted with six, eight, ten, or twelve times its quantity of water, it produces no change in the figure of the vesicles, whose flat shape can then be seen even more distinctly than when mixed with serum itself.

The neutral salts, which, when diluted with water, have been observed to have the effects above described, are Glauber's salt, Epsom salt, a salt formed of the volatile alkali and the vitriolic acid, common nitre, cubic nitre, a salt made with the volatile alkali and the nitrous acid, as well as the salts made with the nitrous acid and magnesia, or

with the nitrous acid and chalk, and alſo common ſalt, digeſtive ſalt of Sylvius, and a ſalt made with vinegar and the foſſil alkali. Theſe experiments were ſufficient to convince me, that this property was very general among thoſe ſalts which conſiſt of acid and alkali; and therefore it ſeemed unneceſſary to proſecute this enquiry farther *.

But acids and alkalies have different effects on theſe veſicles from what neutral ſalts have.

The fixed vegetable alkali, and the volatile alkali, were tried in a pretty ſtrong ſolution, and found to corrugate the veſicles; and in proportion as they were diluted, their effects became ſimilar to water alone, but it is not eaſy to find the point of ſtrength where the veſicles would remain unaltered in the ſolution. And here we may obſerve, that ſince theſe veſicles are found to diſſolve ſo readily in water, and not to be diſſolved in theſe ſolutions of alkali, it is a ſtrong argument againſt their being either oily or ſaponaceous, as they have been ſuſpected.

The effects of acids are very different. I have tried the vitriolic, nitrous, muriatic diſtilled vinegar, and the acid of phoſphorus; theſe, when much diluted, have the ſame effects as water in making the veſicles ſpherical; and, in proportion as they are leſs diluted, they diſſolve the veſicles without making them ſpherical, as water does. I never

* Theſe experiments were made by putting one drop of the ſaturated ſolution of the ſalt into a tea cup, and then adding diſtilled water by a few drops at a time; and to this mixture the ſerum of the blood, highly tinged with the red veſicles, was added.

could

could find any point of dilution where the acids like the neutral salts produced no change on the figure of the vesicles. This experiment is the more to be attended to, as these vesicles have been supposed to be oily and saponaceous, which is improbable, since they dissolve more readily in acids than in alkalies.

Salts made with earth of alum, and any of the acids, always corrugate those vesicles, unless they be very much diluted; when their effects are similar to those of the water alone, that is, they make the vesicles assume a spherical shape. I could not discover any point of strength in these solutions where the particles would remain in them without being changed in their shape.

The same was observed of spirit of wine: some of the metalline salts, as copperas, sublimate, and Roman vitriol, were tried; and when much diluted, their effects were not different from those of water; but in proportion as the solution was stronger, they corrugated the vesicles more and more.

Urine, when containing much of its salts, has effects similar to the serum; but in proportion as it is weaker, its effects are more like those of water.

The use therefore of those salts which enter into the composition of the blood is probably to preserve the red vesicles in their flat form; for we must suppose some advantages attend that shape, since nature has made use of it so generally in the blood of different animals. And as both a very strong solution of neutral salts and a very diluted one alters the shape of the vesicles, it is probable nature has limited

limited the proportions of the water and the ſalts in our blood. A degree of latitude in theſe proportions however ſeems to be admitted; for I obſerved the veſicles equally unchanged when mixed with a ſolution of ſalts conſiſting of eight drops of water to one of the ſaturated ſolution, and when added to a mixture of fifteen drops of water to one of the ſame ſolution.

Not only the neutral ſalts in the blood are capable of preventing the ſerum from diſſolving the veſicles; but the mucilage or lymph with which the ſerum is ſo much impregnated, ſeems to contribute to the ſame effect.

When the veſicles have been made ſpherical, by being mixed with water, if a ſmall quantity of pretty ſtrong ſolution of a neutral ſalt be added, they are immediately ſhriveled, a few of them recover their former flat ſhape, but the greateſt part are contracted irregularly into ſmaller ſpheres. When theſe veſicles thus recover their ſhape, after having been a ſhort time mixed with water, they are generally more tranſparent, and appear thinner, a part of their ſubſtance having been diſſolved in the water; and thence it is more eaſy to diſtinguiſh the little ſolid particle which is contained in them. By this experiment I have had the pleaſure of convincing many curious perſons of the compoſition of this part of the blood, who were not quite ſatisfied from ſome of the other experiments.

I have mentioned above, and have ſhewn in plate XII. that theſe veſicles are of different ſizes in different animals. I have likewiſe obſerved, that they are not all of the ſame ſize in the ſame animal,

ſome

fome being a little larger than others; and fome diffolve in water more readily than others. In the fame fpecies of animals they even differ in fize in the different periods of life. In a chicken on the fixth day of incubation I found them larger than in a full-grown hen, as is reprefented in the plate; and I found them larger in the blood of a very young viper than in that of its mother, out of whofe belly it was taken. I have not however been convinced from experiments, that there is any difference in fize between thofe of a child at its birth, and thofe of an adult perfon.

In the blood of fome infects the veficles are not red, but white, as may eafily be obferved in a lobfter (which Linnæus calls an infect); one of whofe legs being cut off, a quantity of a clear *fanies* flows from it; this, after being fome time expofed to the air, jellies, but lefs firmly than the blood of more perfect animals. When it is jellied, it is found to have feveral white filaments; thefe are principally the veficles concreted, as I am perfuaded from the following experiment.

EXPERIMENT IV. If one of the legs of a lobfter be cut off, and a little of the blood be catched upon a flat glafs, and inftantly applied to the microfcope, it is feen to contain flat veficles that are circular, like thofe of the common fifh, and have each of them a leffer particle in their center as thofe of other animals. But there is a curious change produced in their fhape by being expofed to the air; for foon after they are received on the glafs, they are corrugated; or from a flat fhape are changed into regular fpheres, as is reprefented in plate XII. N° XII.

This

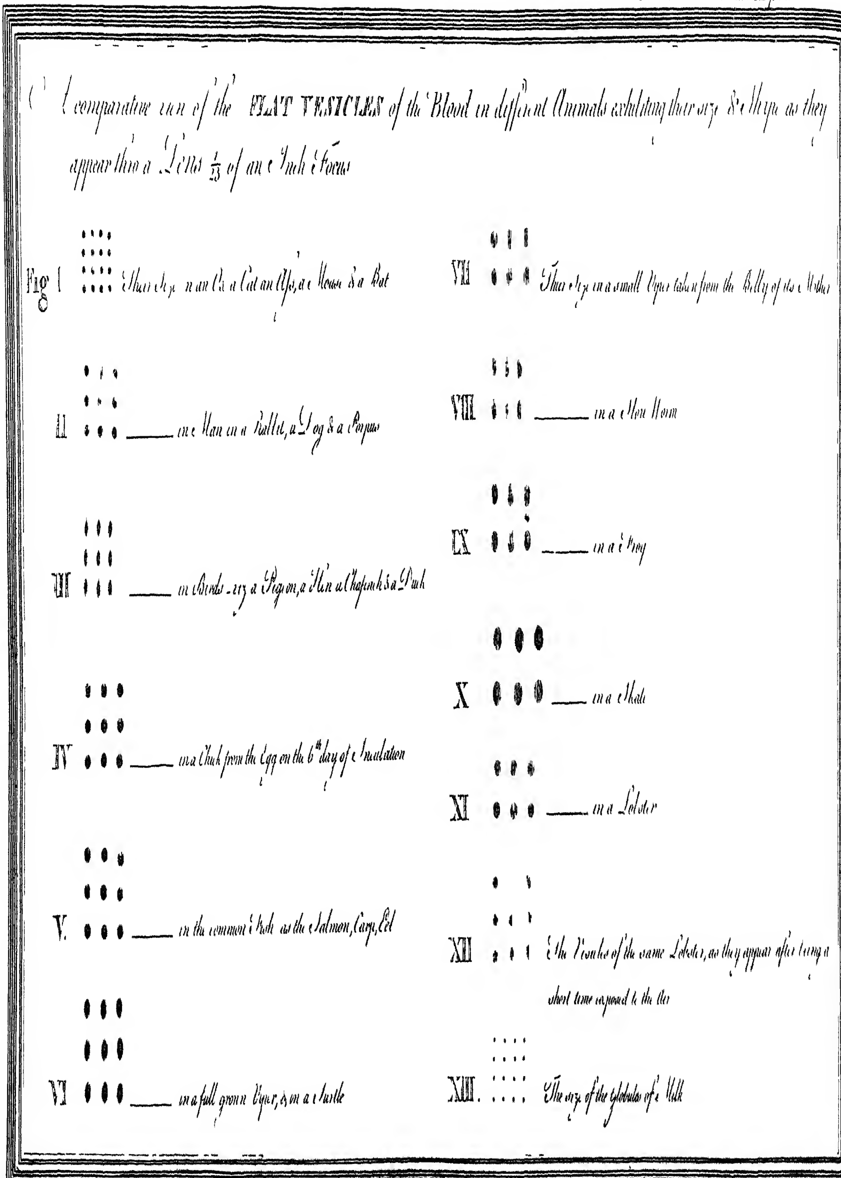
This change takes place ſo rapidly, that it requires great expedition to apply them to the microſcope ſoon enough to obſerve it.

I have obſerved the *ſanies* or blood of a ſhrimp, by cutting off its tail, and found veſicles in it ſimilar to thoſe of the lobſter, which have been a ſhort time expoſed to the air. But I never could apply the blood ſo as to ſee them in their flat form; but, ſince they changed by expoſition to the air, I conjecture that like them they were flat in the blood veſſels; but being more ſuſceptible of changes from the contact of air, they were corrugated before I could get them applied to the microſcope.

The ingenious Leeuwenhoeck has obſerved that in the blood of a graſhopper, its veſicles or globules, as he calls them, are green. I have ſeen the ſame circumſtance in the white caterpillar, whoſe ſerum appeared green when in its veſſels; but when let out from this animal or from a graſhopper, the colour cannot be diſtinguiſhed.

The ſmalleſt animal in which I have diſcerned theſe veſicles is an inſect no bigger than a pin's head, that is ſeen almoſt conſtantly in the river water which we have in London. This inſect, which is a ſpecies of the *Monoculus*, being put into a concave glaſs with a little water, and the rays of the ſun being made to paſs through it, the heart may be ſeen to beat, and the tranſparent blood or *ſanies* found to have a few veſicles, which appear to move one after the other; being made viſible, though tranſparent, by the light paſſing in ſuch a manner as to be refracted by them.

Since



J. Basire Sculp.

Since ſo ſmall an animal as this has theſe curious veſicles equally as the larger and more perfect animals, is it not probable that they are diffuſed through the whole animal creation? And what is found ſo generally amongſt animals muſt be of great uſe in their œconomy.

P. S.

Mr. Hewſon begs leave to add, that ſince theſe experiments open a new field for enquiry; and as he has already ſo far proſecuted the ſubject as to be perſuaded that he has thereby diſcovered not only the uſe of the lymphatic glands and of the Thymus, but alſo of the ſpleen: in order to have the opinions of the ingenious concerning the facts mentioned in this paper, he ventures to ſolicit, that ſuch gentlemen as are curious in natural knowledge, and particularly the Members of the Royal Society, would honour him with their company, at his houſe in Craven-ſtreet, in the Strand, where he will repeat the experiments before them.

XXXIV. *Account of the Effects of a Thunder-Storm, on the 15th of* March 1773, *upon the House of Lord* Tylney *at* Naples. *In a Letter from the Honourable Sir* William Hamilton, *Knight of the Bath, his Majesty's Envoy Extraordinary at the Court of* Naples, *and F. R. S. to* Mathew Maty, *M. D. Sec. R. S.*

Naples, March 20, 1773.

SIR,

Read June 17, 1773.

ON Monday last, about half past ten at night, I had the satisfaction of being one, of many witnesses, to several curious phænomena, occasioned by the lightning having fallen on Lord Tylney's house, in this city. It was on his Lordship's assembly night; so that most of the nobility of this country, many of the foreign ministers, foreigners of distinction, particularly English, were present at the time of the explosion; to be sure there were not less than two hundred and fifty in the apartments, and, including servants, the whole number under Lord Tylney's roof could not be less than five hundred. The lightning passed through nine rooms, seven of which were crouded with

with parties at cards, or converſing; it was viſible in every one, notwithſtanding the quantity of candles, and has left in all, evident marks of its paſſage. Many of the company were ſenſible of a ſmart ſtroke, like that of electricity, and ſome complained for ſeveral days after, of a pain they felt from that ſtroke, but no one received any eſſential hurt; a ſervant, indeed, of the French ambaſſador's houſe has a black mark on his ſhoulder and thigh, from a ſtroke he received on the ſtair-caſe; and another ſervant, who was aſleep on the ſame ſtair-caſe, his head reclining againſt the wall, had the hair entirely ſinged from it on that ſide.

The confuſion at the moment was, as you, Sir, may well imagine, very great: the report, which ſeems to have been equally heard in every room, was certainly as loud as that of a piſtol; and every one flying the room they were in, thinking the danger there, met of courſe in the door-ways, and ſtopped all paſſage. A Poliſh prince, who was playing at cards, hearing the report (as he thought of a piſtol), and feeling himſelf ſtruck, jumped up, and, clapping his hand to his ſword, put himſelf in a poſture of defence. I was ſitting on a card-table, and converſing with Monſieur de Sauſſure, Profeſſor of Natural Hiſtory at Geneva; we happened to be looking different ways, and each of us thought that the bright light and report was immediately oppoſite to us: and, upon enquiry, I found that every one was perſuaded that the greateſt exploſion had been directly before him. I thought that an Indian cracker had been fired, and Monſieur de Sauſſure thought it the report of a piſtol; but hearing, amidſt other

confused cries and noises, a voice saying, *Un fulmine, un fulmine!* we began to examine the gallery in which we were, and soon discovered that the gilding of the cornish had been affected, for in the corners, and at every junction, it was quite blackened; those that had been sitting under the cornishes were covered with the shining particles of the varnish that went over the gilding, and which was thrown off in small dust, at the moment of the explosion. There was a smell of sulphur in Lord Tylney's apartments, but not very considerable; I thought there was more in the apartment above, which Monsieur de Saussure and I visited immediately after, and where we found the same operation had been performed on the gildings. It is very certain that the profusion of gildings, which is remarkable in this house, and the bell-wires, prevented the lightning from making more use of the company to conduct it in its course. I will endeavour to give you as clear an account as I am able, of what I saw the next morning, with Monsieur de Saussure, when we examined together, most carefully, the whole of Lord Tylney's house; which you are at liberty to communicate to our respectable Society, if you think it worthy of its attention. There never was, I believe, an accident of the kind, that proved more clearly, the exact similitude of lightning and the electrical fluid, in all their operations.

The best apartments of this country have usually a broad cornish of lacker, or false gold, round their coved ceilings. Wood, covered with white plaister, a silvered leaf, and a yellow varnish, composes this magnificent cornish; a band of the same sort, but

much

much narrower, goes round the hangings, and down the corners, where it is double, which you will immediately comprehend, by casting your eye on the section of the gallery, marked A, of the inclosed drawing [see TAB. XIII]. The chairs, sophas, frames of pictures, tables, &c. are usually of the same sort of gilding, at least they were so here. By sending you the dimensions of each room of Lord Tylney's apartment, you will see on what a prodigious surface of gilding the lightning spread itself in its course; for you must add the same quantity of gilding in the apartments over Lord Tylney's, which are of the same dimensions, as richly decorated, and as much damaged; though we remarked that Lord Tylney's rooms that had suffered most, did not always correspond with those that suffered most in the upper apartments. Monsieur de Saussure and I began our examination on the flat roof of the house, composed of a kind of stucco, on which there was no sign of damage; neither was there on any of the chimneys. A tin gutter, with many spouts of the same metal, projecting about three feet, is immediately under this roof, and each of the spouts is supported by a small iron rod, or cramp, inserted in the wall underneath, and above by two wires of about the size of a goose-quill, and which likewise go into the wall. We observed a wire of this sort melted; and it seems highly probable, that the lightning found its way into the house at this place. We observed also, from the roof, that, though the house stands high, it is nevertheless commanded by many cupolas, and higher buildings; which, with other circumstances, makes it highly probable, that only a portion

portion of the lightning, the great explofion of which was heard all over Naples, had been conducted through our affembly. In the garrets under the aftiico, or flat roof, we could perceive no figns of damage; under them, in the rich apartment immediately over Lord Tylney's, and confifting of the fame number of rooms, the gilding of the cornifhes, bands, chairs, fophas, &c. exhibited exactly the fame appearance as in Lord Tylney's, which fhall be particularly defcribed prefently. The account of the appearances at the moment of the explofion, given us by the few people that were in the apartments at the time, correfponded perfectly with what we had feen below.

Lord Tylney's apartment confifts of five rooms on a line, and four others, going off at a right angle from the fourth room of that line. The lightning feems to have entered the firft room of the five towards the north, and which is under that part of the gutter where we fufpected it to have entered that part of the houfe. The five rooms of this line are of the fame breadth, 23½ feet, and the four others are 14½ feet broad. The firft room is 18½ feet long. The gilt cornifh of the whole apartment is in general 9½ inches broad, and the gilt band that goes round the hangings 2¾ inches in breadth. The cornifh of the room is only blackened at the joints, particularly the corners, and where there was any flaw or crack in the gilding. The fmall bands, which appear by their colours to have conducted the lightning down from the cornifh in eight different parts of this room are (as in the other rooms) 14 feet

feet high, and theſe are what I ſhall call hereafter vertical deſcents, as marked in the drawing of the ſection of the room A. The gilding of a ſopha in this room is likewiſe blackened. We found, that whenever a chair or ſopha had been affected, it was owing to its having been, at the time of the exploſion, in contact with the gilt band, and that the point of contact was continually marked by a black ſpot, both on the chair and band.

The ſecond room, or gallery, in which Monſieur de Sauſſure and I were at the time the lightning fell, is 33½ feet long; the gilding of the corniſh is much damaged, particularly on that ſide in contact with the bell wire. The lightning in this room had ten vertical deſcents, and paſſed over the gilding of two chairs, two ſopha's, and the frames of two marble tables, the white marble of which, at thoſe parts which were in contact with the gilding, is tinged yellow, and ſuch parts of the damaſk of the chairs and ſopha's as were in contact with the gilding, and had nails underneath, are ſinged. I found, upon enquiry, that moſt of thoſe who were ſitting upon theſe particular chairs and ſopha's were ſenſible of a ſmart ſtroke.

The third room, a ſection of which is marked A. in the drawing, is thirteen feet long, the corniſh much damaged, a ſopha and two chairs damaged; nine vertical deſcents in this room.

The fourth room is 14½ feet long; the corniſh is damaged, and the lightning had nine vertical deſcents in this room likewiſe, the bands being much damaged.

The fifth room is twelve feet two inches long; the cornish much hurt, particularly on the side in contact with the bell wire; there are no vertical bands in this room, therefore no apparent vertical descents of the lightning.

The sixth room, which is the angle to the fourth, is much hurt in the cornish; it is fourteen feet long, and has no gilt bands.

The seventh room nineteen feet ½ long; no signs of damage on the gilding of the cornish, or in any other parts of the room, except the bell wire, which was melted, and seemed alone to have conducted the whole accumulated force of the lightning to the cornish of the next room.

Eighth room; a section of which is represented in the drawing under B. The cornish being overcharged, and the lightning, finding no compleat vertical conductor, jumped from the picture frame over the door to the gilding of the door case, which gilding is six inches wide; and on one side where the gilding ended, it knocked out a piece of wood, which is likewise burnt, or rather singed. The track of the lightning is evidently marked on the white wall, as if by the flame of a candle, black and yellow; and the same sort of tinge is visible (as represented in the drawing) on each side of the gilding of the door. We remarked that the picture over the door was the only one in the room that was in contact, or near the cornish.

The ninth room is eighteen feet long; the cornish is hurt, and the lightning descended from it to a picture frame over the door, and from thence to another, which was the greatest jump that we remarked;

ed; its paſſage is clearly worked on the wall, as is repreſented in the drawing C, and the diſtance from one picture frame to the other is thirteen inches. It went then to the other picture frame, and down to the gilding of the door caſe, which is ſurrounded in part, having made a hole in the wall, which however it did not pierce.

The lightning ſeems to have been much ſtronger in theſe two rooms than in any other; and, as they were ſervants rooms, there were not above two or three people in them at the time of the exploſion.

Underneath theſe apartments we found no traces of the exploſion, except on the wall of the room directly under the door caſe of Lord Tylney's eighth room, where a piece of the plaiſtered wall of about ſix inches ſquare was beat out, and ſcattered about the floor.

Under this room again was a wall in a damp waſh-houſe, where moſt probably the lightning communicated with the earth, and diſperſed itſelf.

Thus have I followed it through its courſe, and will take my leave of you; but firſt I muſt tell you, that I have ſucceeded in diſcharging my battery of nine bottles over the cornices of two of my rooms, which repreſents in miniature exactly what we ſaw in ſuch perfection at Lord Tylney's.

An excellent electrical machine that I had of Mr. Nairne is the wonder of this country; as they

they had never before ſeen electrical experiments in perfection.

I am,

SIR,

with great regard and eſteem,

your moſt obedient

humble ſervant,

Wm. Hamilton.

Explanation of PLATE XIII.

A. Section of the third room. B. Section of the eighth room. C. Section of the ninth room. D. Bands by which the lightning deſcended vertically. E. The gilding affected moſt in the corners and marbled with black, where the lightning was moſt powerful. F. Bell-wire melted. G, Jump of the lightning traced on the white wall. H. Vertical deſcents of the lightning marked on the white walls; the greateſt jump, from the firſt picture frame to the ſecond, is 13 inches. I, Hole made in the wall by the lightning. K. Piece of wood beat out of its place and burnt. L. Sopha, the gilding blackened.

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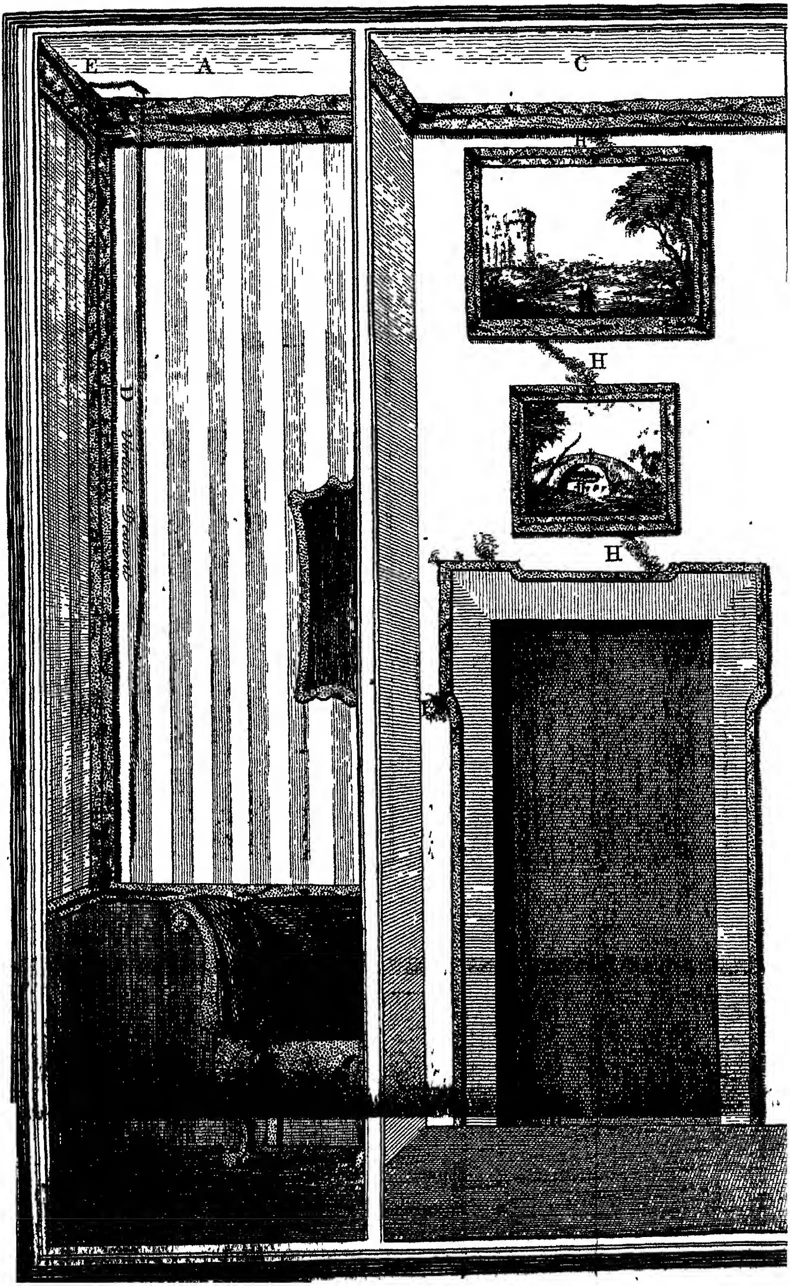

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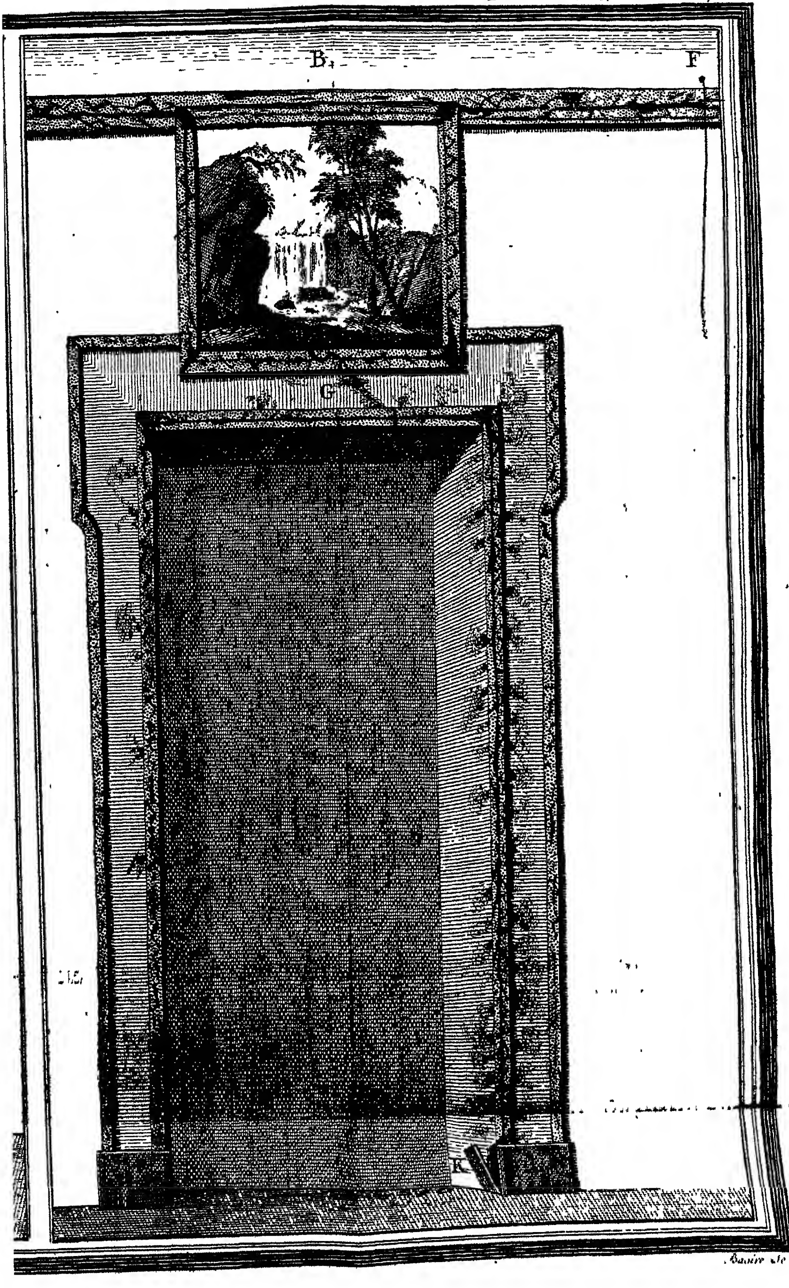


Basire sc.

Receviecl, April 22, 1773.

XXXV. *An Extract of a Letter from Dr.* Nooth *to Dr.* Franklin, *F. R. S. on some Improvements in the Electrical Machine.*

Read June 24, 1773.

IT must undoubtedly appear extraordinary, that, in the present age, when the study of electricity is become so general, and the advances that have been made in the science are so very considerable, I should attempt to recall your attention to the structure of the electrical machine. But I believe it must be allowed, that, notwithstanding the remarkable progress that has of late been made in electrical pursuits, the machine still remained the most imperfect part of the apparatus. The construction of it has been in general left to the workman, who has seldom been in a capacity of making those improvements in it, which it certainly admits of.

The subject, however, seems well worth the attention of the electricians themselves; as a knowledge of the means of correcting the capricious state of their machines will enable them to pursue

their electrical inquiries with more certainty, ſucceſs, and ſatisfaction.

A proſpect of diſcovering the cauſe of the common uncertainty in the action of electrical machines, induced me, ſome months ſince, to make ſome obſervations on the appearances produced by a machine in motion. Being well convinced, that the electric fire, which we receive from a machine, is derived from the cuſhion, and from ſuch parts as communicate with it, I firſt attended to the effects which the glaſs and rubber had on each other.

My inquiries, however, were not directed towards an inveſtigation of the cauſe of that accumulation of electric matter, in conſequence of the friction of the glaſs on the rubber, as I looked on that circumſtance as a ſecret in nature, no leſs impenetrable than gravitation itſelf; but I endeavoured to find out the beſt method of increaſing the excitation of a glaſs, and of taking from it that fire for electrical purpoſes which might be collected on its ſurface.

It is evident, that the electric matter is excited in the inſtant that the glaſs paſſes over the rubber, and that it becomes ſenſible to us by its adhering to the revolving ſurface of the glaſs. It likewiſe appeared to me highly probable, that the quantity of fire, which we find on the glaſs in motion, is not the whole of that which is excited by the paſſage of the glaſs on the rubber. The luminous appearance in the angles between the glaſs and rubber, and which is extremely diſtinct in a dark room, rendered it next to certain, that a part of the excited electric fluid returns immediately to the

cuſhion

cushion without performing a revolution with the glass; and that, of course, a circulation of the fire is thus kept up in the substance of the cushion in the common method of constructing the machines.

To be certainly convinced of this, I attempted to make the passage of the fire from the glass to the anterior part of the cushion, or to that part which corresponds with the ascending side of the cylinder, demonstrable, by placing a piece of silk between the glass and cushion. This silk was larger than the cushion; and part of it was allowed to adhere, by the attraction of the electric fire, to the ascending part of the cylinder. My view in doing this was to cut off, in that part, the immediate communication between the excited glass and cushion, and by that means render the circulation of electric matter visible, which I suspected to take place in the machine; as it was thus forced to turn over the loose edge of the silk before it could return to the cushion. The event answered my expectation; and I then perceived, that the greatest part of the excited fluid was commonly re-absorbed by the forepart of the cushion without becoming sensible on the superior part of the glass.

Having thus verified my supposition by actual experiments with silken flaps of different sizes, I endeavoured to discover a method of preventing that circulation of the electric fluid, and, if possible, of obliging the whole, or the greater part of it, that is once excited, to make the revolution with the glass. This, indeed, the silk, when of considerable breadth, in some measure effected; but I thought that this obstruction to the immediate return

turn of the fire might be rendered more complete by increaſing the thickneſs of the ſilk, or by applying to it ſome nonconducting ſubſtance, that might confine the excited fluid more perfectly to the ſurface of the revolving cylinder.

Bees-wax being a nonconducting ſubſtance eaſily procured, I rubbed the ſilken flap with it, and found, as I expected, that the return of the fire to the cuſhion at the anterior part of the machine was by that means much diminiſhed, and conſequently the excitation of the glaſs was apparently increaſed. The addition, however, of more ſilk was ſtill more effectual in confining the fire to the glaſs; and when it was employed ten or twelve times doubled, it ſeemed to deny any paſſage from the glaſs to the cuſhion.

As I thus diſcovered the method of remedying the common defect in the conſtruction of the anterior part of the cuſhion, I next attended to that part which correſponds with the deſcending ſide of the cylinder. Being convinced that this part of the rubber was alone concerned in the excitation, I imagined that the reverſe of what was neceſſary anteriorly ſhould be adopted in the ſtructure of the poſterior part; that, inſtead of placing nonconducting ſubſtances between the glaſs and cuſhion, we ſhould here make the afflux of the electric matter as great as poſſible, by the application of the moſt perfectly conducting bodies. Confining therefore the amalgam to that place where the glaſs firſt comes in contact with the rubber, I placed ſome tinfoil cloſe to the amalgam, and, bending it back, ſecured it to the metallic plate below the cuſhion. By this means

means the electric matter found an easy access to the place of excitation; and the effect of the machine was thereby incredibly increased. A piece of leather, covered with amalgam, and fixed to the posterior part of the rubber, in such a manner as to allow about an inch of it to pass under the cylinder, answered every purpose of the tinfoil; and, as it was not liable to be corroded by the mercury, like tinfoil, it was on that account much preferable.

From the above experiments it was apparent that the excitation was altogether performed by the posterior portion of the cushion; and that the anterior part, when made of conducting substances, re-absorbs the greater quantity of the excited matter. In the structure therefore of electrical machines, we should always have a free electric communication behind, to facilitate the excitation; and the most perfectly nonconducting substances before, to prevent the re-absorption. To answer these intentions, it will perhaps be adviseable to make the cushion of silk, stuffed with hair, and to lay some metallic conductor round the posterior part, that a free access might be allowed to the electric matter coming to the place of excitation from the inferior part of the machine. Cushions, made in this manner, and then covered with silk ten or twelve times doubled, are much more powerfully excitant than any others that I have yet tried. Various other methods, however, may be pursued in the construction of the rubber; but it should be an invariable rule, to place nonconducting bodies before, and conducting substances behind, the cylinder. From the preceding principles, it follows, that the support to

to the rubber ſhould likewiſe have its conducting and nonconducting ſide. For this purpoſe, it may be neceſſary to employ baked wood, and to cover the poſterior half with tinfoil. The place of excitation will be thus ſufficiently ſupplied with electric matter; and the cylinder will not be robbed of a part of the excited fire, before that fire has made a revolution with the glaſs.

By attending to the place where the excitation is effected, it muſt appear evident, that the amalgam is only to be laid on the poſterior part of the cuſhion; its preſence, indeed, would be uſeleſs, if not injurious, in any other ſituation. It will, however, be found ſomewhat difficult to confine the pure amalgam to the poſterior part of the rubber; but if it is mixed with a little hair-powder and pomatum, it pretty perfectly keeps its place. The ſtrewing the amalgam thus prepared on the glaſs, as it revolves, is perhaps the beſt method of applying it; as, by that means, it is in a great meaſure prevented from paſſing on to the nonconducting ſubſtances that are placed before. Should any of the amalgam be carried forward by the revolution of the glaſs, it ſhould be carefully removed. The neceſſity of keeping that part free from conducting bodies cannot be too much inſiſted on; and, when freſh amalgam is applied as before mentioned to the proper part of the rubber, the flap ſhould be held down during half a dozen turns of the machine, leſt it might collect ſome of the amalgam before it is properly fixed. It is a probable conjecture, that, when the flap of ſilk is covered with amalgam, part of the amalgam, which is not immediately ſubſervient to the excitation,

tion, acts as a conductor in reſtoring the fire again to the cuſhion; and that thus, by an improper diſpoſition of it, we ſuppreſs, inſtead of increaſing, the quantity of the excited matter.

In ſhort, when an electrician attends to the preceding principles in the conſtruction of his rubber, and to the proper diſpoſition of the amalgam, he has nothing to fear from the humidity of the atmoſphere, as his machine will work equally well in all kinds of weather. The reſt of the electrical apparatus may be made according to the directions that have been given by the different electrical writers. Each has had his favourite machine; and, perhaps, no one has been yet contrived that has not had its peculiar advantages.

Received May 13, 1773.

XXXVI. *Properties of the Conic Sections; deduced by a compendious Method. Being a Work of the late* William Jones, *Esq; F. R. S. which he formerly communicated to Mr.* John Robertſon, *Libr. R. S. who now addreſſes it to the Reverend* Nevil Maſkelyne, *F. R. S. Aſtronomer Royal.*

S I R,

Read June 24, 1773.

YOU well know that the curves formed by the ſections of a cone, and therefore called CONIC SECTIONS, have, from the earlieſt ages of geometry, engaged the attention of mathematicians, on account of their extenſive utility in the ſolution of many problems, which were incapable of being conſtructed by any poſſible combination of right lines and circles, the magnitudes uſed in plane geometry. The properties of theſe curves are become far more intereſting within the two laſt centuries, ſince they have been found to be ſimilar to thoſe which are deſcribed by the motions of the cœleſtial bodies in the Solar ſyſtem.

Two

Two different methods have been taken by the writers who have treated of their properties; the one, and the more antient, is to deduce them from the properties of the cone itself; the other is to consider the curves, as generated by the constant motion of two or more strait lines moving in a given plane, by certain laws.

There are various methods of generating these curve lines *in plano*; one method will give some properties very easily; but others, with much trouble: while, by another mode of description, some properties may be readily derived, which, by the former, were not so easily come at: so that it appears there may be a manner of describing the curves similar to the Conic Sections, by the motion of lines on a plane, which, in general, shall produce the most essential properties, with the greatest facility.

That excellent mathematician, the late William Jones, Esq; F. R. S. had drawn up some papers on the description of these curves, or lines of the second kind, very different from what he gave in his *Synopsis Palmariorum Matheseos*, published in the year 1706; or from that of any other writer on this subject. A copy of these papers he was pleased to let me take about the year 1740. He had not finished them as he intended; but, in their present state, they appear of too much consequence to be lost; as, it is much to be feared, his own copy, together with many other valuable papers, are; and therefore, I am desirous of preserving them in the Philosophical

Tranfactions, in the manner I at firft tranfcribed them; although, I am aware, they might have been put into a form more pleafing to the generality of readers: I have indeed annexed larger diagrams than what accompanied the author's copy, in order to render the lines more diftinct, as all the relations are to be reprefented in a fingle figure, of each kind.

Mr. Jones, having laid down a very fimple method of defcribing thefe curves, feems to have been defirous of arriving at their properties in as expeditious a way as he could contrive; and therefore he has ufed the algebraic method, in general, of reducing his equations; and, on fome occafions, has ufed the method of fluxions, to deduce fome properties chiefly relating to the tangents; and, by a judicious ufe of thefe, he has very much abridged the fteps which otherwife he muft have taken, to have deduced the very great variety of relations he has obtained: thefe he intended to have arranged in tables, from whence an equation expreffing the relation between any three or more lines of the Conic Sections, might be taken out as readily as a logarithm out of their tables; this he has only partly executed; but it may eafily be continued by thofe who are defirous to have it done, and are fufficiently acquainted with what follows.

From the Houfe of the Royal Society,
April 29, 1773.

THE

THE DESCRIPTION OF LINES OF THE SECOND KIND.

LET the right lines AD, AQ, be drawn on a plane, at any inclination the one with the other. See PLATE XIV. Fig. 1, 2, 3.

In AD, AQ, take Aa, AM, of any given magnitude, and draw MN parallel to AD.

On the points A, a, let two rulers AP, aP, revolve, and cut MN, AQ, in N and Q, ſo that AQ be every-where equal to MN.

Then ſhall the interſection P of the rulers deſcribe lines of the ſecond kind, or curves of the firſt kind.

Where the right-line Aa, is the firſt, or tranſverſe diameter.

The point C, biſecting the diameter Aa, is the center.

The right-line PD, drawn parallel to AQ, is the ordinate to the diameter Aa.

The part AD, or CD, of the diameter, is the abſciſs, when reckoned to begin from A to C, or from C to A.

The right line Bb drawn from the center C parallel to the ordinate PD, and terminated in the curve, is called the ſecond, or conjugate diameter.

Thoſe diameters to which the ordinates are perpendicular, are called the axes.

And AM is the parameter to the diameter Aa.

THE

THE PROPERTIES OF LINES OF THE SECOND KIND.

1. Put $Aa = d = 2AC = 2t$; $Bb = \delta = 2BC = 2c$; $AM = 2p$; $PD = y$; $CD = x$; $AD = u$. Then $\overline{PD}^2 = \frac{p}{t} \times ADa$.

Or $yy = \frac{p}{t} \times u \times \overline{d \mp u} = 2pu \mp \frac{p}{t}uu = \frac{p}{t} \times \overline{\pm tt \mp xx} = \pm pt \mp \frac{p}{t}x^2$.

For $PD = \frac{AM \times AD}{MN} = \frac{AQ \times Da}{Aa}$ (by sim. $\triangle$s). Th. $\overline{PD}^2 = \frac{AM}{Aa} \times ADa$.

2. Consequently $\frac{t}{p}yy = \pm tt \mp xx = du \mp uu = \pm\frac{1}{4}dd \mp xx$.

3. Hence $\frac{t}{p}y\dot{y} = \mp x\dot{x} = \overline{t \mp u} \times \dot{u} = x\dot{u}$.

4. And $pt = cc$, or $2pd = \delta\delta$; for when $y = c = \frac{1}{2}\delta$, then $x = 0$.

5. Therefore $\frac{\overline{PD}^2}{ADa} = \frac{yy}{\pm tt \mp xx} = \frac{p}{t} = \frac{2p}{d} = \frac{2pd}{dd} = \frac{pt}{tt} = \frac{\delta\delta}{dd} = \frac{cc}{tt} = \frac{\overline{BC}^2}{\overline{AC}^2}$.

6. The curve line whose property is $yy + \frac{p}{t}uu - 2pu = 0$,

(where the abscissa begins at the curve),

Or $yy + \frac{cc}{tt}xx - cc = 0$, (where it begins at the center),

is called an Ellipsis. This curve returns into itself. For when $x = 0$, then $y = c$; and when $y = 0$, then $x = t$. Which can happen but two ways.

7. The curve line whose property is $yy - \frac{p}{t}uu - 2pu = 0$,

Or $yy - \frac{cc}{tt}xx + cc = 0$, is called an Hyperbola. This curve spreads out infinitely. For y increases as x increases; and has four legs tending contrary ways: for xx, or yy, may be produced as well from $-x$, or $-y$; as from $+x$, or $+y$.

8. If the point a, is supposed to be at an infinite distance from A, so that a ruler aP moves in a parallel position to AD; then is $yy = 2pu$, or $yy - 2pu = 0$, the property of the curve described, and is called a Parabola. This curve spreads out infinitely; for y increases as u increases.

9. Let

9. Let Aa, Pp, be any two firſt diameters; Bb, Qq, their ſecond diameters. PLATE XIV. Fig. 4.

Draw the ordinates PD, QE, to the diameter Aa, and the ordinate Pd, to the diameter B b.

Let PT be a tangent, and PM be perpendicular, to the curve, in P; PT cutting Aa, Bb, produced, in T, t; and PM, in M, m.

Put the ſubtangent DT $= s$, $dt = \sigma$.

Let AT $= r$, PM $= \pi$, CE $= w$, CT $= q = x \pm s = t \pm r$.

Put s, s', = ſine and coſine of the angle MPD, or angle PmB.

R = tabular radius.

Then $\text{CDT} = xs = \overline{t \mp u} \times s = \frac{t}{p} yy = \frac{tt}{cc} yy = \pm tt \mp xx = u \times \overline{d \mp u} = \text{AD}a$.

For $\dot{u} \times \overline{t \mp u} = \left(\frac{t}{p} y\dot{y} =\right) \alpha y\dot{y}$. Therefore $\left(\frac{\dot{u}}{\dot{y}} = \frac{\alpha y}{t \mp u} =\right) \frac{\alpha y}{x} = \frac{s}{y}$ (by ſim. Δs).

And $\text{C}dt = y\sigma = \frac{p}{t} xx = \frac{cc}{tt} xx = \pm cc \mp yy = \text{B}db$. For $\left(\frac{\dot{u}}{\dot{y}} =\right) \frac{\alpha y}{x} = \frac{x}{\sigma}$.

10. Hence $\overline{\text{AC}}^2 = tt = x \times \overline{x \pm s} = x \times \overline{t \pm r} = xq = \text{DCT}$.

11. And $\overline{\text{BC}}^2 = cc = y \times \overline{y + \sigma} = y \times \text{C}t = d\text{C}t$.

12. Conſequently $\frac{\overline{\text{P}d}^2}{\text{B}db} = \frac{xx}{\pm cc \mp yy} = \frac{tt}{cc} = \frac{\overline{\text{AC}}^2}{\overline{\text{BC}}^2}$.

13. Alſo $\overline{\text{CE}}^2 = ww = \left(\overline{\text{DT}}^2 \times \frac{\overline{\text{QE}}^2}{\overline{\text{PD}}^2} = \overline{\text{DT}}^2 \times \frac{\text{AE}a}{\text{AD}a} =\right) ss \times \frac{tt - ww}{sx} = sx = \frac{tt}{cc} yy = \text{AD}a = \text{CDT}$.

14. Therefore $\text{AE}a = \left(\frac{\overline{\text{CE}}^2}{\overline{\text{DT}}^2} \times \text{AD}a = \frac{sx}{ss} \times sx =\right) xx = \overline{\text{CD}}^2$.

15. And $\overline{\text{QE}}^2 = \left(\frac{\overline{\text{PD}}^2}{\text{AD}a} \times \text{AE}a =\right) \frac{cc}{tt} xx = \frac{p}{t} xx$.

In the general schemes. PLATE XV. and Fig. 5. PLATE XIV.

16. Let Aa, Bb, be the longest and shortest axes.
Draw $C\varrho$ perpendicular to the tangent PT, cutting it in ϱ.
Put $CP = T$; $CQ = C$; $C\varrho = \varrho$.

Then $C\varrho = \varrho = \left(\frac{ct \times CE}{CQ} = \frac{cc}{yy} \times \frac{ty}{cC} = \right) \frac{tc}{C} = \frac{AC \times CB}{CQ}$.

Hence the parallelogram, under the two axes, is equal to the parallelogram under any two diameters.

17. Draw the tangents AN, an, to any vertices A, a, meeting any diameters Pp, Qq, produced in V, U, and v, u, and the tangent PT in N, n.

Then $AU = \left(\frac{CA \times EQ}{CE} = \right) \frac{px}{p}$. And $AV = \left(\frac{CA \times PD}{CD} = \right) \frac{ty}{x}$.

18. Also $CU = \left(\frac{AU \times CQ}{EQ} = \right) \frac{cC}{y}$. And $CV = \left(\frac{CP \times CA}{CD} = \right) \frac{tT}{x}$.

19. Hence $PV = (CV \backsim CP =)\ T \times \frac{t \backsim x}{x}$. And $pV = T \times \frac{t+x}{x}$.

Also $QU = (CU \backsim CQ =)\ C \times \frac{c \backsim y}{y}$. And $qU = C \times \frac{c+y}{y}$.

20. When Aa and Bb are the longest and shortest axes; and when $y = p$,
Then $xx = tt \mp \frac{t}{p} yy$ will become $tt \mp pt = tt \mp cc$, which call ff.
And $CD = x$, will become $CF = Cf = f$.
The points F, f, are called the Focii.

21. Hence $AF = af = \pm t \mp f$; $Af = aF = t + f$.

22. Also $\overline{CF}^2 = \overline{Cf}^2 = ff = \pm tt \mp cc = \pm tt \mp pt$.
And in the ellipsis, $\overline{AC}^2 = \left(\overline{BC}^2 + \overline{CF}^2 = \right) \overline{BF}^2$.
in the hyperbola, $\overline{CF}^2 = \left(\overline{AC}^2 + \overline{BC}^2 = \right) \overline{BA}^2$.
Hence, a circle described from B, with the distance AC in the ellipsis, or from C, with the distance AB in the hyperbola, will cut the axis Aa in the focii F, f.

23. Draw

23. Draw FP, fP, from the focii F, f, to any point P of the curve; and draw the conjugate diameters Pp, Qq.

Put $PF = z$; $Pf = v$; $PC = T$; $QC = C$; $\frac{f}{t} = \varphi$; $\frac{f}{c} = \gamma$. Then

$$\overline{Pf}^2 = vv = yy + xx + 2xf + ff = tt + 2fx + \frac{ff}{tt}xx = TT + ff + 2fx.$$

$$\overline{PF}^2 = zz = yy + xx - 2xf + ff = tt - 2fx + \frac{ff}{tt}xx = TT + ff - 2fx.$$

$$\text{For } yy = \left(\frac{cc}{tt} \times \overline{tt \backsim xx} = \right) \frac{tt \backsim ff}{tt} \times \overline{tt \backsim xx}.$$

24. $$Pf = v = \left(\sqrt{tt + 2fx + \frac{ff}{tt}xx} = \right) t + \frac{f}{t}x = t + \varphi x = \frac{tt + fx}{t}.$$

$$PF = z = \left(\sqrt{tt - 2fx + \frac{ff}{tt}xx} = \right) t - \frac{f}{t}x = t - \varphi x = \frac{tt - fx}{t}.$$

25. $PF \pm Pf = z \pm v = 2t = Aa.$

26. $\overline{Pf}^2 + \overline{PF}^2 = vv + zz = 2yy + 2xx + 2ff = 2tt + 2\varphi\varphi xx = 2TT + 2ff.$

27. $\overline{Pf}^2 - \overline{PF}^2 = vv - zz = 4fx = \overline{v + z} \times \overline{v - z} = 2t \times \overline{v \mp z}.$

28. $$FPf = zv = z \times \overline{2t \mp z} = 2tz \mp zz = tt \backsim \varphi\varphi xx = \overline{tt \backsim xx} \times \frac{cc}{tt}xx$$

$$= cc \backsim \frac{ff}{cc}yy = \frac{tt}{cc}yy + \frac{cc}{tt}xx = \overline{CE}^2 + \overline{EQ}^2$$

$$: \overline{CQ}^2 = CC = T \times P.$$

29. Let $$m = t - z = v - t = \frac{fx}{t} = \varphi x = \pm \tfrac{1}{2}v \mp \tfrac{1}{2}z$$

$$= \frac{fx}{\sqrt{cc + ff}} = \frac{x}{t}\sqrt{tt \mp cc} = \sqrt{tt \mp zv}$$

$$= \sqrt{tt \mp CC} = \sqrt{ff + cc - zv}.$$

30. Hence $$\dot{z} = -\dot{v} = \varphi\dot{x} = -\frac{f}{t}\dot{x} = \frac{fy}{px}\dot{y}.$$

And $$\dot{v} = -\dot{z} = \varphi\dot{x} = \frac{f}{t}\dot{x} = -\frac{fy}{px}\dot{y}.$$

31. $\mathrm{AC} = t = \tfrac{1}{2}z \pm \tfrac{1}{2}v = \frac{cc}{p} = \sqrt{cc \pm ff} = \frac{fx}{m} = \frac{cx}{\sqrt{cc \backsim yy}}$

$$= \frac{c}{y}\sqrt{sx} = \sqrt{x \times \overline{x \backsim s}} = \frac{y^2 + \sqrt{y^4 + 4p^2x^2}}{2p} = \frac{yy}{2p} + \sqrt{\frac{y^4}{4p^2} + xx}.$$

32. $\overline{\mathrm{CD}}^2 = \overline{\mathrm{P}d}^2 = xx = \overline{\frac{ttyy}{ccs}}\Big|^2 = \overline{\frac{tyy}{ps}}\Big|^2 = \pm tt \mp \frac{t}{p}yy = \frac{tt}{cc} \times \overline{cc \mp yy}$

$$= \frac{ff + cc}{cc} \times \overline{cc \mp yy} = tt \mp \frac{tt}{cc}yy = \frac{mm}{\varphi\varphi} = \frac{tt}{ff}mm$$

$$= \frac{cc + ff}{ff}mm = \frac{tt}{ff} \times \overline{tt \mp zv} = \frac{tt}{ff} \times \overline{tt \mp \mathrm{CC}} = \overline{\frac{tt}{q}}\Big|^2$$

$$= \frac{tt}{cc} \times zv - \frac{tt}{cc}yy = \frac{t}{p} \times zv - \frac{t}{p}yy = \frac{tt}{ff} \times \overline{t - z}\big|^2$$

$$= \frac{tt}{ff} \times \overline{v - t}\big|^2 = \overline{\frac{tt \mp tz}{f}}\Big|^2 = \overline{t \mp u}\big|^2 = \overline{\sqrt{tt + \tfrac{1}{4}ss} - \tfrac{1}{2}s}\Big|^2$$

$$= \frac{tt}{ff} \times \overline{tt \mp \frac{tt}{cc}\pi\pi} = \frac{t^4}{c^4} \times \frac{\mathrm{SS}}{\mathrm{RR}}\pi\pi.$$

33. And $\dot{x} = -\frac{ty\dot{y}}{px} = -\frac{tty\dot{y}}{ccx} = -\frac{tt\dot{y}}{qq} = -\frac{t\dot{z}}{f} = \frac{t\dot{v}}{f}.$

34. $\overline{\mathrm{PD}}^2 = \overline{\mathrm{C}d}^2 = yy = \frac{p}{t}sx = \frac{cc}{tt}sx = \frac{cc}{tt} \times \overline{\pm tt \mp xx} = \pm cc \mp \frac{cc}{tt}xx$

$$= \pm pt \mp \frac{p}{t}xx = \frac{2cc}{t}u \mp \frac{cc}{tt}uu = \frac{\pm tt \mp ff}{tt} \times \overline{\pm tt \mp xx}$$

$$= \frac{cc}{tt} \times zv - \frac{cc}{tt}xx = \frac{cc}{tt} \times \mathrm{CC} - \frac{cc}{tt}xx = \frac{cc}{ff} \times \overline{zv - cc}$$

$$= \frac{cc}{ff}nn = \frac{pt}{ff}nn \text{ (putting } nn = zv - cc\text{)}$$

$$= \frac{tt\pi\pi - c^4}{ff} = cc \mp \frac{tt}{cc} \times \frac{\mathrm{SS}}{\mathrm{RR}}\pi\pi = \frac{\mathrm{S'S'}}{\mathrm{RR}}\pi\pi.$$

35. And $\dot{y} = -\frac{px\dot{x}}{ty} = -\frac{ccx\dot{x}}{tty} = \frac{px\dot{z}}{fy} = -\frac{px\dot{v}}{fy}.$

36. Also $\frac{ff}{cc}yy = zv - cc = \frac{ff}{tt} \times \overline{tt \mp xx} = ff \mp \frac{ff}{tt}xx$

$$= nn = ff \mp mm = \mathrm{CC} - cc.$$

37. $\overline{\mathrm{FC}}^2$

3

37. $\overline{PC}^2 = TT = xx + yy = xx \pm cc \mp \frac{cc}{tt}xx = tt - ff + \frac{ff}{tt}xx = cc + \varphi\varphi xx$
$= cc + tt \backsim zv = cc + mm = cc + tt \backsim CC$
$= tp + \varphi\varphi xx = 2cc + ff \backsim zv = 2pt + ff \backsim zv.$

38. And $\dot{T} = \frac{\varphi\varphi x\dot{x}}{T} = \frac{\varphi\varphi x\dot{x}}{\sqrt{tp + \varphi\varphi xx}} = \left(\frac{\varphi\varphi x}{T} \times - \frac{ty\dot{y}}{px} = \right) \frac{ffy\dot{y}}{ptT} = - \frac{ffy\dot{y}}{ccT}.$

39 Alſo $\overline{CQ}^2 + \overline{CP}^2 = (cc + tt =) \overline{CB}^2 + \overline{CA}^2.$

40. $CT = q = \frac{tt}{x} = \left(tt \times \frac{f}{tm} = \right) \frac{ft}{m} = \frac{ft}{t \backsim z} = \frac{ft}{\sqrt{tt \backsim zv}}$

$Ct = \frac{cc}{y} = \frac{cf}{n} = \frac{cf}{\sqrt{zv - cc}}.$

41. And $\dot{q} = - \frac{q\dot{x}}{x} = - \frac{qq\dot{x}}{tt} = \frac{qqy\dot{y}}{ccx} = \frac{qqy\dot{y}}{ptx} = - \frac{tt\dot{x}}{xx}$ (for $tt = qx$).

42. $AT = (\pm CT \mp CA =) \pm \frac{tt}{x} \mp t = \frac{t}{x} \times \pm \overline{t \mp x} = \pm \frac{ft}{m} \mp t = \pm \frac{ft}{t \backsim z} \mp t.$

$aT = (CT + Ca =) \frac{tt}{x} + t = \frac{t}{x} \times \overline{t + x} = \frac{ft}{m} + t = \frac{ft}{t \backsim v} + t.$

43. $FT = \left(\pm CT \mp CF = \pm \frac{ft}{m} \mp f = \frac{\pm ft \mp fm}{m}\right) = \frac{fz}{t \backsim z} = \frac{fz}{m} = \frac{tz}{x}.$

$fT = \left(CT + Cf = \frac{ft}{m} + f = \frac{ft + fm}{m} = \right) \frac{fv}{v \backsim t} = \frac{fv}{m} = \frac{tv}{x}.$

On Aa deſcribe a circle, draw the tangent TP′, and draw CP′.
Continue DP to P′, and at right angles to TP′, draw FR′,

Then $FR' = \left(\frac{FT}{CT} \times CP' = \frac{tz}{x} \times t \times \frac{x}{tt} = \right) z = FP.$

$fr' = \left(\frac{fT}{CT} \times CP' = \frac{tv}{x} \times t \times \frac{x}{tt} = \right) v = fP.$

 44. DT

44. $DT = s = \frac{tyy}{px} = \frac{ttyy}{ccx} = \frac{\pm tt \mp xx}{x} = \frac{tnn}{fm}$.

$dT = \sigma = \frac{pxx}{ty} = \frac{ccxx}{tty} = \frac{cc \mp yy}{y} = \frac{cmm}{fn}$.

45. $AN = \left(\frac{Ct}{CT} \times AT =\right) \frac{cm}{nx} \times \overline{t - x} = \frac{c}{n} \times \overline{f -}$

$an = \left(\frac{Ct}{CT} \times aT =\right) \frac{cm}{nx} \times \overline{t + x}$.

46. $VN = \left(\frac{PV \times Ct}{PC} = \frac{t - x}{T} \times \frac{T}{z} \times \frac{fc}{z} =\right) \frac{fc}{nx} \times \overline{t - x} = \frac{fc}{nm} \times \overline{f - \sqrt{tt - zv}}$

$\frac{fc}{\sqrt{zv - cc} \times \sqrt{tt - zv}}$

$N = Ct = \frac{cc}{y}$.

47. $AD = \left(\pm AC \mp CD = \pm t \mp \frac{tm}{f} =\right) \frac{t}{f} \times \pm \overline{f \mp m}$.

$aD = \left(AC + CD = t + \frac{tm}{f} =\right) \frac{t}{f} \times \overline{f + m}$.

48. Produce PF, pf, to meet the curve in Π, π;

Draw $\Pi\Delta$ perpendicular to Aa.

Put $F\Pi = z'$, $f\pi = v'$, $F\Delta = x'$.

Now, $FD = (\pm CF \mp CD =) \pm f \mp x = \pm f \mp \frac{tm}{f} = \frac{tz - cc}{f}$

$fD = f + \frac{tm}{f} = \frac{tv - cc}{f}$.

Then $F\Delta = x' = (C\Delta - CF =) \frac{tm}{f} - f = \left(\frac{t}{f} \times \overline{t - z'} - f =\right) \frac{cc - tz'}{f}$

$f\Delta = f + \frac{tm}{f} = \frac{tv' - cc}{f}$.

49. From

49. From F, f, draw FR, fr, perpendicular to the tangent PT, and cutting it in R, r.

$$\text{FR} = \lambda = \frac{\text{FT} \times \text{C}\varrho}{\text{CT}} = \frac{cz}{c} = \frac{cz}{\sqrt{zv}} = \frac{cz}{\sqrt{2tz - zz}} = c\sqrt{\frac{z}{v}}$$

$$= c\sqrt{\frac{t - \varphi x}{t + \varphi x}} = \sqrt{\frac{ptz}{zt - z}} = \frac{z\varrho}{t}$$

$$fr = \lambda' = \frac{fT \times \text{C}\varrho}{\text{CT}} = \frac{cv}{c} = \frac{cv}{\sqrt{zv}} \cdots\cdots = c\sqrt{\frac{z}{v}}$$

$$= c\sqrt{\frac{t + \varphi x}{t - \varphi x}} = \sqrt{\frac{ptv}{2t - v}}.$$

$$\dot{\lambda} = \frac{tc\dot{z}}{v\sqrt{zv}}.\quad \text{For } \lambda^2 = \frac{c^2 z}{v}.\quad \text{Th. } 2\lambda\dot{\lambda} = \left(\frac{c^2 v\dot{z} - c^2 z\dot{v}}{vv} = \right)\frac{2c^2 t\dot{z}}{vv}.$$

50. $$\text{TR} = \left(\frac{\text{AT} \times \text{RT}}{\text{AN}} = \frac{tnz}{mc} = \right)\frac{ttnz}{fcx} = \frac{ttn}{fx}\sqrt{\frac{z}{v}} = \frac{tn}{m}\sqrt{\frac{z}{v}}.$$

$$\text{T}r = \frac{a\text{T} \times rf}{an} = \frac{tn}{m}\sqrt{\frac{v}{z}}.$$

51. Draw PM perpendicular to the tangent PT, meeting the axes Aa, Bb, in M, m.

$$\text{DM} = \left(\frac{\text{PD} \times \text{PD}}{\text{TD}} = \right)\frac{ccm}{ft} = \frac{cc}{ft}\sqrt{tt \pm zv} = \frac{cc}{tt}x = \frac{p}{t}x$$

$$= \frac{\text{S}}{\text{R}}\tau = p \mp \frac{p}{t}u = \frac{cc}{tf}\sqrt{tt \mp \frac{tt}{cc}\pi\pi}.$$

$$dm = \left(\frac{\text{PD} \times \text{P}d}{\text{DM}} = \right)\frac{ttn}{fc} = \frac{tt}{cc}\sqrt{zv - cc} = \frac{tt}{cc}y = \frac{t}{p}y.$$

52. $$\text{CM} = \left(\text{CD} \mp \text{DM} = x \mp \frac{cc}{tt}x = \right)\frac{tt \mp cc}{tt}x = \frac{ff}{tt}x = \frac{f}{t}m.$$

$$= \frac{f}{t}\sqrt{tt \mp zv} = \frac{f}{t} \times t \mp z = \frac{f}{t} \times \sqrt{tt \mp \frac{tt}{cc}\pi\pi}$$

$$cm = \left(dm \mp cd = \frac{tt}{cc}y \mp y = \right)\frac{tt \mp cc}{cc}y = \frac{ff}{cc}y = \frac{f}{c}n = \frac{f}{c}\sqrt{zv - cc}$$

53. $$\text{FM} = \left(\text{CF} \backsim \text{CM} = f \backsim \frac{fm}{t} = \right)\frac{f}{t} \times \overline{t \mp m} = \frac{fz}{t} = \pm f \mp \frac{ff}{tt}x.$$

$$f\text{M} = \left(fc + \text{CM} = f + \frac{fm}{t} = \right)\frac{f}{t} \times \overline{t + m} = \frac{fv}{t} = f + \frac{ff}{tt}x.$$

54. $\overline{Fm}^2 = \overline{fm}^2 = (\overline{f}^2 + \overline{cm}^2 =) ff + \frac{ffnn}{cc} = \frac{ffzv}{cc} = \frac{ff}{cc} \times CC.$

55. $AM = (AC \backsim CM =) t \backsim \frac{fm}{t} = \frac{tt \backsim fm}{t} = t \backsim \frac{ff}{tt} x.$

$aM = (aC + CM =) t + \frac{fm}{t} = \frac{tt + fm}{t} = t + \frac{ff}{tt} x.$

56. $TM = (TF + FM = \frac{fz}{m} + \frac{fz}{t} =) \frac{fzv}{tm} = \frac{zv}{x} = \frac{CC}{x}$

$tm = (Ct + Cm = \frac{fc}{n} + \frac{fn}{c} =) \frac{fzv}{cn} = \frac{zv}{y} = \frac{CC}{y}.$

57. $PM = \pi = \left(\frac{FR \times TM}{FT} =\right) \frac{c}{t}\sqrt{zv} = \frac{c}{t}C = \frac{p}{c}C = \frac{c}{t}\sqrt{tt \backsim \phi^2 x^2}$

$= \frac{c}{t}\sqrt{\overline{t+\phi x} \times \overline{t-\phi x}} = \frac{cc}{tt} \times \frac{R}{S} x = \frac{cR}{tS}\sqrt{ce \mp yy}$

$= \frac{c}{tt}\sqrt{t^4 - ffxx} = \frac{c}{t}\sqrt{cc + \gamma\gamma yy} = \frac{1}{t}\sqrt{c^4 + ffyy}$

$= \frac{c}{t}\sqrt{2tz \mp zz} = \sqrt{2pz \mp \frac{p}{t} zz}.$

$Pm = \pi' = \left(\frac{PM \times Pd}{DM} =\right) \frac{t}{c}\sqrt{zv} = \frac{t}{c}C = \frac{t}{p}C = \frac{t}{c}\sqrt{tt \backsim \phi\phi xx}$

$= \frac{t}{c}\sqrt{\overline{t+\phi x} \times \overline{t-qx}} = \frac{1}{c}\sqrt{t^4 \backsim ffxx} = \frac{t}{c}\sqrt{cc + \gamma\gamma yy}$

$= \frac{t}{cc}\sqrt{c^4 + ffyy} = \frac{t}{c}\sqrt{2tz \mp zz} = \sqrt{\frac{2tt}{p} z \mp \frac{t}{p} zz}.$

58. $Mm = (Pm \mp PM = \frac{t}{c}C \mp \frac{c}{t}C =) \frac{tt \mp cc}{ct} C = \frac{ff}{ct}\sqrt{zv}.$

$= \frac{ff}{ct}\sqrt{tt \backsim \phi\phi xx}.$

59. $PT = \tau = \left(\frac{PD \times PM}{DM} =\right) \frac{n}{m}C = \sqrt{\frac{zv - cc}{tt - zv} \times zv} = \frac{tn}{fx}C.$

$= \frac{ty}{cx}C = \frac{ty}{cx}\sqrt{tt \backsim \phi\phi xx} = \frac{\sqrt{\overline{zv - cc} \times zv}}{t \mp z}.$

$Pt = \tau' = \left(\frac{PT \times Pd}{DT} =\right) \frac{m}{n}C = \sqrt{\frac{tt - zv}{zv - cc} \times vz} = \frac{fx}{tn}C$

$= \frac{cx}{ty}C = \frac{cx}{ty}\sqrt{tt - \phi\phi xx}.$

60. TN

60. $TN = \left(\dfrac{PT \times TA}{DT} = \dfrac{nc}{m} \times \dfrac{t}{m} \times \overline{f \backsim m} \times \dfrac{fm}{tnn} = \right) \dfrac{fc}{mn} \times \overline{f \backsim m} = \dfrac{cc}{xy} \times \overline{t \backsim x}$

$Tn = \left(\dfrac{PT \times Ta}{DT} = \dfrac{nc}{m} \times \dfrac{t}{m} \times \overline{f+m} \times \dfrac{fm}{tnn} = \right) \dfrac{fc}{mn} \times \overline{f+m} = \dfrac{cc}{xy} \times \overline{t+x}$

61. $PN = \left(\dfrac{AD \times PT}{DT} = \dfrac{t}{f} \times \overline{f \backsim m} \times \dfrac{nc}{m} \times \dfrac{fm}{tnn} = \right) \dfrac{c}{n} \times \overline{f \backsim m} = \dfrac{cc}{ty} \times \overline{t \backsim x}$

$Pn = \left(\dfrac{aD \times PT}{DT} = \dfrac{t}{f} \times \overline{f+m} \times \dfrac{nc}{m} \times \dfrac{fm}{tnn} = \right) \dfrac{c}{n} \times \overline{f+m} = \dfrac{cc}{ty} \times \overline{t+x}$

62. $PR = \left(\dfrac{PT \times TM}{TM} = \right) \dfrac{nz}{c} = n\sqrt{\dfrac{z}{v}} = \sqrt{\dfrac{z}{v} \times \overline{zv - cc}} = \dfrac{f}{c} y \times \sqrt{\dfrac{z}{v}}$

$= \dfrac{f}{c} y \times \sqrt{\dfrac{t - \varphi x}{t + \varphi x}} = \dfrac{f}{cc} \times xy$

$Pr = \left(\dfrac{PT \times fM}{TM} = \right) \dfrac{nv}{c} = n\sqrt{\dfrac{v}{z}} = \sqrt{\dfrac{v}{z} \times \overline{zv - cc}} = \dfrac{f}{c} \times y \sqrt{\dfrac{v}{z}} = \dfrac{f}{c} y \times \sqrt{\dfrac{t + \varphi x}{t - \varphi x}}$

63. $T\varrho = \left(\dfrac{PT \times TC}{TM} = \right) \dfrac{ntt}{mc} = \dfrac{tt}{t - z}\sqrt{\dfrac{zv - cc}{vz}} = \dfrac{fytt}{mcc} = \dfrac{yt^3}{xcc}$

64. $P\varrho = \left(T\varrho \mp TP = \dfrac{ntt}{mc} \mp \dfrac{nc}{m} \times \right) \dfrac{n}{mc} \times \overline{tt \mp cc} = \dfrac{n}{mc} \times mm = \dfrac{nm}{c}$

65. $Tt = \left(\dfrac{PT \times Ct}{PD} = \dfrac{nc}{m} \times \dfrac{cf}{n} \times \dfrac{f}{cn} = \right) \dfrac{ffc}{mn} = ff\sqrt{\dfrac{}{\overline{tt - zv} \times \overline{zv - cc}}}$

$= \dfrac{tt}{xy} c = \dfrac{tc}{xy}\sqrt{tt - \dfrac{ff}{tt} xx}$

66. $Nn = \left(\dfrac{PT \times Aa}{DT} = \right) \dfrac{2fc}{cn} = \dfrac{2c}{y} c = \dfrac{2c}{y}\sqrt{zv} = 2f\sqrt{\dfrac{zv}{zv - cc}}$

From C and F, draw $c\gamma$ and $F\epsilon$, perpendicular to Pf

Put $P\epsilon = b$; Σ = *co*-fine of the $\angle\, Af P$; R = tabular radius.

Then $f\gamma = \left(\dfrac{cf \times fD}{Pf} = \dfrac{zv - cc}{f} = \right) \dfrac{zv - cc}{v} = \left(\dfrac{\Sigma}{R} \times Cf\right) \dfrac{\Sigma f}{R}$

And $Pf = v = \dfrac{cc}{t - \dfrac{\Sigma}{R} f} = \dfrac{tt - ff}{t - \dfrac{\Sigma}{R} f}$

$$P. = h = \left(\frac{\pm zz + vv \mp 4ff}{2v} = \frac{\pm TT \mp ff}{v} = \frac{\pm 2pt \mp vz}{v} = \right)$$

$$= \pm \frac{2pt}{v} \mp z = \pm \frac{z+v}{v} p \mp z,$$

Hence $p = \frac{\pm h \mp z}{z+v} \times v$; $v = \frac{pz}{\mp h \pm z - p}$; $z = \frac{p-h}{v-p} \times v$

Therefore p is leſs than, equal to, or greater than $\pm h \mp z$, in the Ellipſis, Parabola, or Hyperbola.

67. Draw $m\mu$, making the $\angle$ P$m\mu$ = $\angle$ PCM, and cutting PC in μ.

Then $P\mu = \left(\frac{PM \times Pm}{PC} = \right) \frac{CC}{T} = \frac{zv}{\sqrt{tt \pm cc \mp zv}} = \frac{tt - \phi\phi xx}{\sqrt{cc + \phi\phi xx}}$

$= P = \frac{1}{2}$ parameter to PC.

68. Let FΦ be an ordinate to the axis Aa. at the focus F. and ΦG a tangent to the curve in Φ, meeting Aa, BC, AN, an, in G, g, S, s. Then

$$F\Phi = \sqrt{\frac{cc}{tt} \times AFa} = \sqrt{\frac{cc}{tt} \times cc} = \Big) \frac{cc}{t} = \frac{pt}{t} = p$$

$= \frac{1}{2}$ parameter to Aa.

69. $CG = \frac{tt}{f} = \frac{ff \pm cc}{f} = \frac{tt}{\sqrt{tt \mp cc}} = \frac{tt}{\sqrt{t \times t - p}}$

70. $FG = (CG \backsim CF = \frac{tt}{f} \backsim f = \frac{tt \backsim ff}{f} = \frac{cc}{f} = \frac{pt}{f}.$

71. Draw PH parallel to Aa; and GH perpendicular to Aa, meeting PH, PT, in H, h; then

$PH = DG = (CG \backsim DC =) \frac{tt}{f} \backsim x = \frac{\ }{f} z = \frac{\ }{f} \times PF.$

72. TO

72. $\text{T}\Theta = (\text{CT} \backsim \text{CG} = \frac{ft}{m} \backsim \frac{tt}{f} = \frac{t}{m} \times \overline{f \backsim x} =) \frac{t}{t-z} \times \frac{\pm tz \mp cc}{f} = \frac{tt}{fx} \times \overline{f \backsim x}.$

73. $\text{G}h = \left(\frac{\text{PD} \times \text{TG}}{\text{DI}} = \frac{cn}{f} \times \frac{t}{m} \times \overline{f \backsim x} \times \frac{fm}{tnn} = \frac{\text{c}}{n} \times \overline{f \backsim x} =\right)$

$$\frac{cc}{fy} \times \overline{f \backsim x} = \frac{c}{\sqrt{zv - cc}} \times \frac{tz - cc.}{f}$$

74. $\text{P}h = \left(\frac{\text{TP} \times \text{DG}}{\text{DI}} =\right) \frac{z}{n}\text{c} = \frac{cz}{fy}\text{c} = z \times \sqrt{\frac{zv}{zv - cc}} = \frac{cz}{fy} \sqrt{zv}.$

75. $\text{R}h = (\text{P}h - \text{PR} =) \frac{z\text{c}}{n} - \frac{zn}{\text{c}} = \frac{z}{n\text{c}} \times \overline{\text{cc} - nn} =)$

$$\frac{}{n\text{c}} - \frac{zcc}{\sqrt{zv - cc + zv}} - \frac{zc^3}{fy\text{c}}$$

76. $\text{F}h = (\sqrt{\overline{\text{FR}}^2 + \overline{\text{R}h}^2} =) \frac{cz}{n} = \frac{cz}{\sqrt{zv - cc}} = \frac{ccz}{fy} = \frac{ptz.}{fy}$

77. Let $\text{D}\Sigma$ be any Ordinate to the axe $\text{A}a$, cutting the curve in Σ, and the focal tangent ΦG in σ;

Then $\text{D}\sigma = \left(\frac{\text{T}\Phi + \text{DG}}{\text{IG}} = \frac{cc}{t} \times \frac{tz}{f} \times \frac{f}{cc} =\right) z = \text{FP} = \text{F}\Sigma.$

78. Therefore $\text{AS} = \text{AF}$; $as = a\text{F}$; $\text{C}g = \text{CA}$; by sim. Δs.

79. $\overline{\text{DP}}^2 = (\overline{\text{D}\sigma}^2 - \overline{\text{D}\Sigma}^2 = \overline{\text{D}\sigma + \text{D}\Sigma} \times \overline{\text{D}\sigma - \text{D}\Sigma} =) \text{P}\sigma \times \sigma\Sigma.$

79. Let the tangents PN, p L, to the opposite vertices P, p, cut the tangents AN, an, to the opposite vertices A, a, in N, n, L, l.
Then Pn = pL; an = AL; PN = pl; AN al.
For the Trapezia's PCan, pCAL, are similar and equal;
And so are the Trapezia's PCAN, pCal.

81. $\mathrm{PF} \times \mathrm{M}f = \left(\frac{f}{t} \times vz =\right) \mathrm{P}f \times \mathrm{MF} = \frac{f}{t} \times \mathrm{FP}f.$

82. $\mathrm{CEQ} = (xy =)\ \mathrm{CDP} = d\mathrm{CD} = \mathrm{DM} \times dm.$

83. $\mathrm{AN} \times a\mathrm{D} = \frac{ctn}{f} = an \times \mathrm{AD} = \mathrm{AC} \times \mathrm{PD} = \mathrm{D}al = \mathrm{DAL}.$

84. $\mathrm{RP}r = nn = \mathrm{TP}\varrho.$

85. $\mathrm{RT}r = \left(\frac{nn}{mm} \times tt = \frac{tt - xx}{xx} \times tt =\right) \mathrm{AT}a.$

86. $\mathrm{RT}f = \left(\frac{tnz}{m\mathrm{C}} \times \frac{fv}{m} = \frac{ft}{m} \times \frac{n\mathrm{C}}{m} =\right) \mathrm{CTP} = r\,\mathrm{TF}.$

87. $\mathrm{NT}n = \left(\frac{ff\mathrm{CC}}{mm\,nn} \times \overline{ff \backsim mm} =\right) \frac{ff\mathrm{CC}}{mm} = \mathrm{PT}f.$

88. $\mathrm{PN} \times \mathrm{CB} = \left(\overline{t \backsim x} = \frac{fc}{tn}\mathrm{C} =\right) \mathrm{AN} \times \mathrm{CQ}.$

89. $\mathrm{CAV} = \left(t \times \frac{ty}{x} = \frac{tt}{x} \times y =\right) \mathrm{CT} \times \mathrm{PD}.$

90. $\mathrm{PR}h = \left(\frac{cczz}{\mathrm{CC}} =\right) \overline{\mathrm{FR}}^2.$

Hence Fh is perpendicular to FP.

91. $\mathrm{FM}f = \left(\frac{fz}{t} \times \frac{fv}{t} =\right) \frac{ff}{tt} \times zv = \frac{ff}{tt} \times \mathrm{CC} = \frac{ff}{tt} \times \overline{\mathrm{CQ}}^2$

92. $\mathrm{AM}a = \left(tt \backsim \frac{ffmm}{tt} =\right) cc \times \frac{ff}{tt} \times zv$

$= \overline{\mathrm{CB}}^2 + \frac{ff}{tt} \times \overline{\mathrm{CQ}}^2 = \overline{\mathrm{CB}}^2 + \mathrm{FM}f.$

93. $\frac{\overline{PN}^2}{\overline{TN}^2}=\frac{xx}{tt}=\frac{\overline{CD}^2}{\overline{AC}^2}=\frac{\overline{AN}^2}{\overline{VN}^2}=\frac{\overline{CA}^2}{\overline{CT}^2}=\frac{ADa}{ATa}=\frac{ADa}{RTr}=\frac{\overline{AD}^2}{\overline{AT}^2}=\frac{\overline{aD}^2}{\overline{aT}^2}=\frac{CDT}{ATa}$.

94. $\frac{FT}{CT}=\frac{z}{t}=\frac{FP}{CA}=\frac{FR}{C\varrho}$. Sim. Δs TFR, TCϱ.

And $\frac{AD}{DT}=\frac{x}{t+x}=\frac{CD}{aD}=\frac{AC}{aT}$.

95. $\frac{fT}{CT}=\frac{v}{t}=\frac{fP}{AC}=\frac{fr}{C\varrho}$. And $\frac{AT}{DT}=\frac{t}{t+x}=\frac{AC}{aD}=\frac{CT}{aT}$.

96. $\frac{PM}{Pm}=\frac{cc}{tt}=\frac{p}{t}=\frac{\overline{BC}^2}{\overline{AC}^2}$. And $\frac{Pm}{fm}=\frac{t}{f}=\frac{AC}{CF}$.

97. $\frac{PMf}{\overline{PM}^2}=\frac{ff}{cc}=\frac{\overline{t+c}\times\overline{t-c}}{cc}=\left(\frac{tt-tp}{tp}=\right)\frac{t-p}{p}=\frac{\overline{CF}^2}{\overline{CB}^2}$.

98. Let PM, the perpendicular to the tangent PT, cut the axis Aa in M; and fP, produced, cut FR in ϕ.
Then will PM bisect the angle FPf.
For PF $\times$ Mf = Pf $\times$ MF.

99. And the angle FPT is equal to the angle fPt.
For $\angle$TPF + $\angle$FPM = $\angle t$Pf + $\angle f$PM.

100. Therefore PT will bisect the angle FPϕ.
For $\angle\phi$PT = ($\angle f$Pt =) $\angle$FPT.

101. Consequently Pϕ = PF. And Rϕ = RF.

102. A circumference of a circle described from t, with the radius tN, will cut the axis Aa in the focii F, f.
For TN $\times$ Tn = TF $\times$ Tf.

103. A

103. A circumference of a circle described from C, with the radius CA, will cut the tangent PT in R, r.

Whence the perpendiculars RF, rf, to that tangent, will cut the axis Aa in the focii F, f.

For $TR \times Tr = TA \times Ta$.

104. A circumference described from B, with AC, in the Ellipsis, or from C, with AB in the Hyperbola, will cut the axis Aa, in the focii F, f.

For, in the Ellipsis, $tt = cc + ff$, or $\overline{AC}^2 = (\overline{BC}^2 + \overline{CF}^2 =) \overline{BF}^2$.

And in the Hyperbola, $ff = tt + cc$, or $\overline{CF}^2 = (\overline{AC}^2 + \overline{BC}^2 =) \overline{AB}^2$.

105. Let CQ produced, cut PF, Pf, in z, x; draw mz, mx, and MZ, MX, parallel to them, cutting PF, Pf in Z, X.

Then $Px = CR = CA = Pz = t$.

106. Hence $\angle Pxz = \angle Pzx$; and $\angle mxz = \angle mzx$.

For Pm is perpendicular to zx.

Consequently, the angles Pzm, Pxm; PZM, PXM are equal.

107. And the triangles Pzm, Pxm, are similar and equal:

And so are the triangles PZM, PXM.

Consequently, the trapezias Pzmx, PZMX, are similar.

108. Let CR, Cr, cut PF, Pf, in K, k.

Then $CK = \left(\frac{CF \times Pf}{Ff} =\right) \frac{1}{2} Pf = Pk = kr$.

And $Ck = \left(\frac{Cf \times PF}{Ff} =\right) \frac{1}{2} PF = PK = KR$.

109. Also $PX = PZ = \left(\frac{PM}{Pm} \times Pz = \frac{cc}{t} \times \frac{c}{tc} \times t =\right) \frac{cc}{t} = \left(\frac{pt}{t} =\right) p$.

110. The

110. The Trapezias $\text{FG}b\text{R}$, PDFR, $frtc$, are similar, and consequently their corresponding parts are proportional.

That is, $\left\{\begin{array}{l} \frac{\text{FG}}{\text{PD}} = \frac{\text{G}h}{\text{DF}} = \frac{b\text{R}}{\text{FR}} = \frac{\text{RF}}{\text{RP}} \\ \overline{fr} = \overline{rt} = \overline{tc} = \overline{cf} \end{array}\right\}$

For the triangles $\text{RF}b$, RPF, and $\text{F}b\text{G}$, FPD, are similar.

111. The Trapezias $\text{C}g\text{PD}$, $t\text{CMP}$ are similar, and consequently their corresponding parts are proportional.

That is, $\frac{\text{C}g}{t\text{C}} = \frac{g\text{P}}{\text{CM}} = \frac{\text{PD}}{\text{PM}} = \frac{\text{DC}}{\text{P}t}$.

112. And CR, $\text{C}r$, are parallel to $\text{P}f$, PF, and equal to CA.

For $\text{RT}f = \text{CTP} = r\text{TF}$.

113. Let $\Sigma' =$ sine of the $\angle t\text{P}f$, or TPF; $\text{R} =$ tabular radius.

Then $\frac{\text{R}}{\Sigma'} = \left(\frac{\text{P}f}{fr} =\right) \frac{\text{C}}{c} = \frac{\sqrt{zv}}{c}$.

Put $\Sigma =$ sine of the $\angle \text{PCQ}$, made by any diameter and its ordinate.

Then $\frac{\text{TT}}{\text{CC}} = \frac{1}{2} ff \pm \sqrt{\frac{1}{4} f^4 \backsim \frac{r^2 t t^2}{\Sigma^2}}$; $\frac{tt}{cc} = \frac{1}{2}\text{FF} \pm \sqrt{\frac{1}{4}\text{F}^4 \backsim \frac{\text{T C} \Sigma^2}{rr}}$.

114. Let the parallels $\text{PF}\Pi$, $\text{C}\beta r$, $pf\pi$ be drawn, cutting the curve in Π, β, π; and ordinately applied to some diameter (2τ), whose parameter is 2π, and semi-conjugate $\text{C}\beta = \chi$, to which $\text{P}\pi$ is ordinately applied at δ.

Then $\text{C}\delta = \left(\frac{\text{PF} + \pi f}{2} = \frac{\text{PF} + \text{F}\Pi}{2} =\right) \frac{1}{2} z + \frac{1}{2} z' = \frac{1}{2}\text{P}\Pi = \left(\frac{\overline{\text{C}\beta}^2}{\text{C}r} =\right) \frac{\overline{\text{C}\beta}^2}{\text{AC}} = \frac{\chi\chi}{t} = \frac{\pi\tau}{t}$.

115. $\overline{\text{C}\beta}^2 = \left(\frac{1}{2}\text{AC} \times \text{P}\Pi =\right) \frac{1}{2} t \times \overline{z + z'} = \left(\overline{\text{AC}}^2 \times \frac{\text{PF}\Pi}{\text{A}a} =\right) \frac{tt}{cc} z z' = \frac{t}{p} z z'$.

116. $\frac{\text{FP}}{\text{F}\Pi} = \frac{z}{z'} = \frac{\text{FD}}{\text{F}\Delta} = \frac{tz - cc}{cc - tz'} = \frac{z - p}{p - z'}$.

117. $\text{PF}\Pi = \frac{pzz}{2z - p} = \frac{1}{2} p \times \overline{z + z'} = \frac{1}{2} p \times \text{P}\Pi$.

118. $\text{P}\Pi = z + z' = \frac{2zz}{2z - p} = \frac{2\pi\tau}{t} = \frac{\tau}{t} \times 2\tau$.

$$\overline{\mathrm{CB}}^2 = cc = pt = \pm tt \mp ff = \pm \overline{t \mp f} \times \overline{t + f} = \mathrm{AF}a = \mathrm{VAU} = t\mathrm{C}d = \mathrm{CFG}$$

$$= \mathrm{PM} \times \mathrm{C}\varrho = \mathrm{FR} \times fr = \mathrm{AN} \times an = al \times \mathrm{AL} = \mathrm{AC} \times \mathrm{F}\Phi = tt \times \frac{\mathrm{PM}}{\mathrm{P}m}$$

$$= tt \times \frac{\mathrm{DM}}{\mathrm{DC}} = tt \times \frac{a\mathrm{C}}{dm} = tt \times \frac{\overline{\mathrm{PM}}^2}{\overline{\mathrm{CQ}}^2} = tt \times \frac{\overline{\mathrm{PD}}^2}{\mathrm{AD}a} = tt \times \frac{\mathrm{PD}}{\mathrm{TDC}} = ff \times \frac{}{\mathrm{C}m}$$

$$: \mathrm{AM}a - \Gamma\mathrm{M}f = \mathrm{FP}f - \mathrm{RP}r = ff \times \frac{t}{}$$

$$\mathrm{PN}$$

$$= \mathrm{TT} + \mathrm{CC} - tt = \frac{tt}{xx} \times \overline{zv + xx - tt} = \tfrac{1}{2} zv \pm \sqrt{\tfrac{1}{4} z^2 v^2 ffyy}.$$

$$\overline{\mathrm{CQ}}^2 = \mathrm{CC} = \mathrm{P} \times \mathrm{T} = \mathrm{MP}m = \mathrm{NP}n = \mathrm{TP}t = \mathrm{FP}f = \mathrm{CP}\eta = \mathrm{L}pl$$

$$= \mathrm{PN} \times p\mathrm{L} = \mathrm{CD} \times \mathrm{TM} = \mathrm{PD} \times tm = \frac{tt}{cc} \times \overline{\mathrm{PM}}^2 = \frac{cc}{tt} \times \overline{\mathrm{P}m}^2 = \frac{cc}{ff} \times \overline{fm}^2$$

$$= \frac{tt}{ff} \times \mathrm{FM}f = \overline{\frac{ct}{ff} \times \mathrm{M}m}\Big|^2 = \overline{\frac{\mathrm{ACB}}{\mathrm{C}\varrho}}\Big|^2 = \overline{\mathrm{PM}}^2 + \mathrm{FM}f = \overline{\mathrm{BC}}^2 + \mathrm{RP}r$$

$$= tt + cc - \mathrm{TT} = tt - \frac{ff}{tt} xx = \frac{tt}{cc} yy + \frac{cc}{tt} xx = \frac{\mathrm{DC}}{\mathrm{DT}} \times \overline{\mathrm{PT}}^2.$$

$$\overline{\mathrm{CF}}^2 = ff = tt - cc = tt - tp = \frac{cc}{p} \times \overline{t - p} = \frac{tt}{xx} \times mm = \mathrm{P}\varrho \times \mathrm{T}t$$

$$= \frac{tt}{\mathrm{CC}} \times \mathrm{FM}f = \frac{cc}{\mathrm{CC}} \times \overline{fm}^2 = \frac{mm}{\mathrm{CC}} \times \mathrm{FT}f = \frac{mm}{\mathrm{CC}} \times \mathrm{NT}n.$$

The ſemi-parameter (p) to the greater axis ($\mathrm{A}a$) is equal to

$$\mathrm{F}\Phi = \mathrm{PZ} = \frac{\mathrm{AF}a}{\mathrm{AC}} = \frac{\mathrm{V}a\mathrm{U}}{\mathrm{AC}} = \frac{\mathrm{CFG}}{\mathrm{AC}} = \frac{t\mathrm{C}d}{\mathrm{AC}} = \frac{\mathrm{PF}\Pi}{\frac{1}{2}\mathrm{P}\Pi} = \frac{\mathrm{PM} \times \mathrm{C}\varrho}{\mathrm{AC}}$$

$$= \frac{\mathrm{FR} \times fr}{\mathrm{AC}} = \frac{\mathrm{AN} \times an}{\mathrm{AC}} = \frac{\mathrm{AC} \times \mathrm{PM}}{\mathrm{P}m} = \frac{\mathrm{AC} \times \mathrm{DM}}{\mathrm{DC}} = \frac{\mathrm{AO} \times d\mathrm{C}}{dm} = \frac{\mathrm{BC} \times \mathrm{PM}}{\mathrm{CQ}}$$

$$= \frac{\mathrm{BC} \times \mathrm{CQ}}{\mathrm{P}m} = \frac{\overline{\mathrm{BC}}^2}{\mathrm{AC}} = \frac{cc}{t} = \frac{ttyy}{sx} = \frac{\pm tt \mp ff}{t} = \frac{cc - yy}{xx} \times t$$

$$= \frac{\mathrm{CC} + \mathrm{TT} - tt}{t} = \frac{\mathrm{CC} + xx - tt}{xx} \times t = \frac{2t - z}{tz} \times \overline{\mathrm{FR}}^2 = \frac{2t - v}{tv} \times \overline{fr}^2.$$

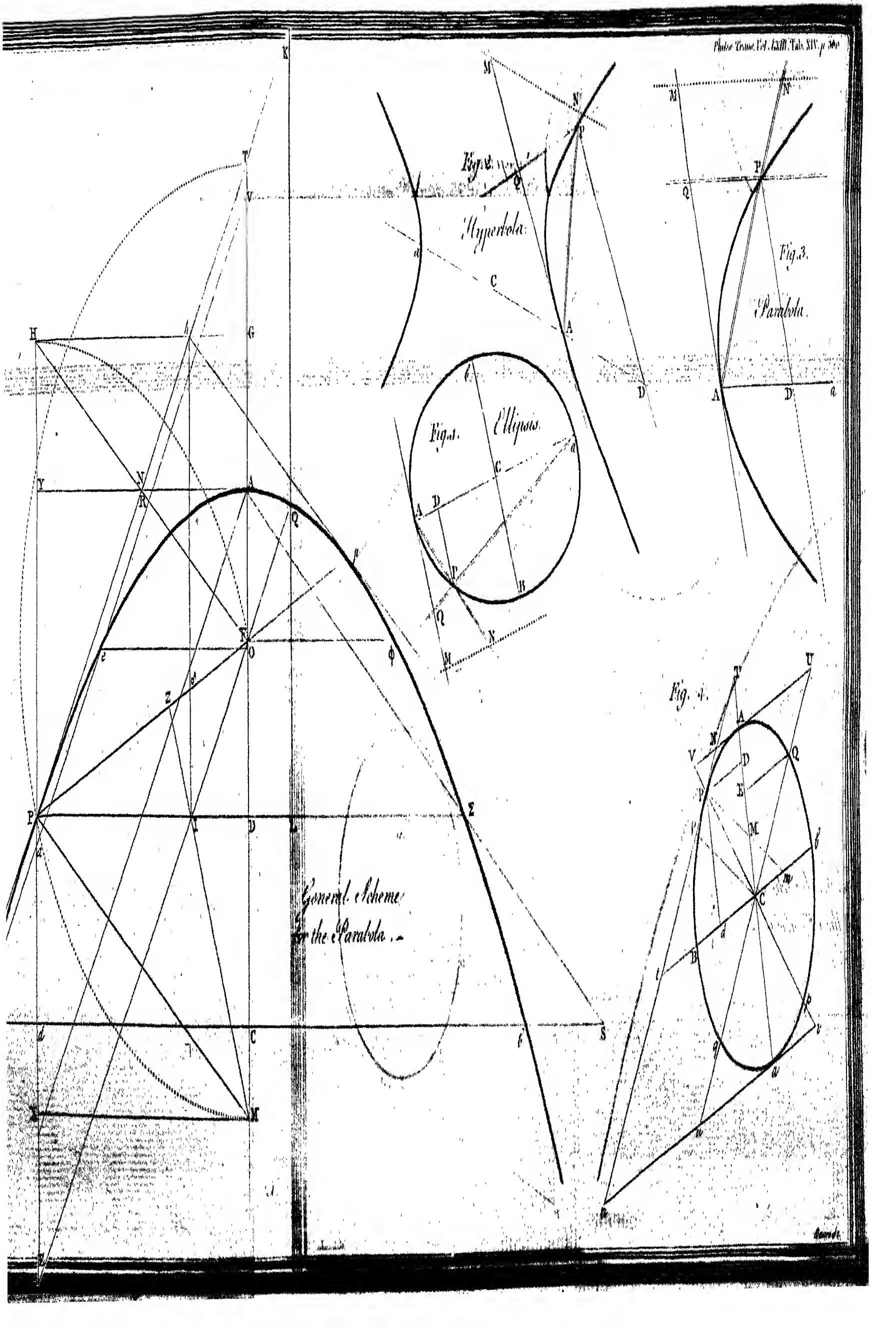
The ſemi-parameter (P) to any diameter ($\mathrm{P}p$) is equal to

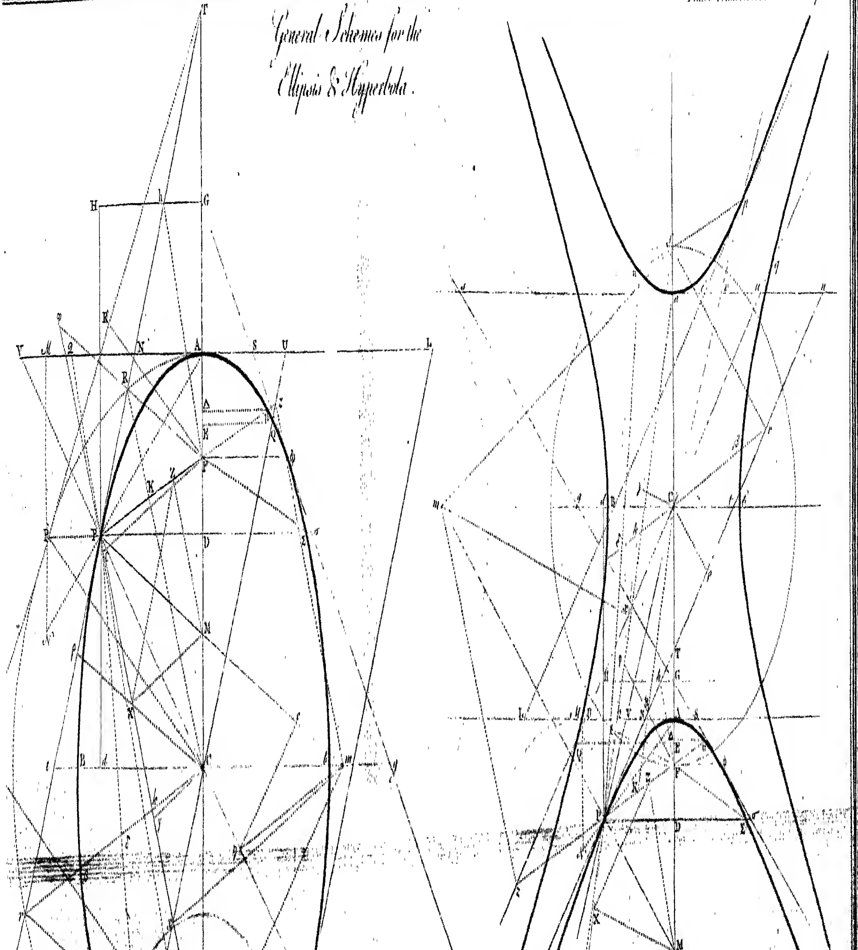
$$\mathrm{P}\eta = \frac{\overline{\mathrm{CQ}}^2}{\mathrm{PC}} = \frac{\mathrm{CC}}{\mathrm{T}} = \frac{\mathrm{MP}m}{\mathrm{PC}} = \frac{\mathrm{NP}n}{\mathrm{PC}} = \frac{\mathrm{TP}t}{\mathrm{PC}} = \frac{\mathrm{FP}f}{\mathrm{PC}} = \frac{\mathrm{L}pl}{\mathrm{PC}} = \frac{\mathrm{PN} \times p\mathrm{L}}{\mathrm{PC}}$$

$$= \frac{\mathrm{CD} \times \mathrm{TM}}{\mathrm{PC}} = \frac{\mathrm{PD} \times tm}{\mathrm{PC}} = \frac{\mathrm{PT} \times \mathrm{PN}}{\mathrm{PV}} = \frac{tt}{cc} \times \frac{\overline{\mathrm{PM}}^2}{\mathrm{PC}} = \frac{cc}{tt} \times \frac{\mathrm{P}m^2}{\mathrm{PC}}$$

$$= \frac{cc}{ff} \times \frac{\overline{fm}^2}{\mathrm{PC}} = \frac{tt}{ff} \times \frac{\mathrm{PM}f}{\mathrm{PC}} = \frac{tt + cc - \mathrm{TT}}{\mathrm{T}}.$$ [See TAB. XIV, XV.]

XXXVII. *An*


Philos. Trans. Vol. LXIII. Tab. XIV. p.
Fig. 2.
Hyperbola.
Fig. 3.
Parabola.
Fig. 1.
Ellipsis.
Fig. 4.
General Scheme for the Parabola.



General Schemes for the Ellipsis & Hyperbola.

XXXVII. *An Essay, towards elucidating the History of the Sea-Anemonies, by Abbé* Dicquemare, *Member of several Academies, Professor of Experimental Philosophy,* &c. *at* Havre de Grace. *Communicated by* Matthew Maty, M. D. *Sec. R. S. Translated from the* French.

Read Feb. 11, and April 29, 1773.

WHatever is relative to animals, their mode of being, growth, and decrease, propagation, strength, actions, distempers, food, length of life, and even the phænomena observed at their death, are so many objects interesting to man. If his moral qualities bear no analogy with theirs, his natural constitution admits of similarities between them.

Memoire pour servir à l'histoire des Anemones de Mer; par Mr. l'Abbé Dicquemare, *de plusieurs Académies Royales, Professeur de Physique Expérimentale, au* Havre de Grace.

TOUT ce qui a rapport aux animaux, leur maniere d'être, les accroissemens et décroissemens qu'ils éprouvent, leur génération, leur force, leurs actions, leurs maladies, leur nourriture, la durée de leur vie, les phénomenes qu'ils nous présentent même à la mort, sont autant d'objets qui doivent interesser l'homme. Si son être moral n'offre avec eux aucune analogie, sa constitution physique permet des similitudes.

Comparative anatomy, and experiments made upon living animals, have unravelled to us part of their vital, natural, and animal faculties. Among the laſt, thoſe which depend upon the organic diſpoſition of the parts, and which perhaps are the principle of many more functions, might receive new light from obſervations to be made upon ſuch animals, as ſeem to deviate moſt from our mode of being. Surprized by the phænomena theſe animals afford, we pry with eager looks into the ſecrets of nature; there we perceive effects, which ſhew us, that the inmoſt conformation of ſome particular animals not tallying with our former notions, we muſt renounce, as groundleſs prepoſſeſſions, ſuch laws as we had been too haſty in laying down as univerſal. Thoſe effects, and the method of obſerving them, which ought, ſeemingly, upon a firſt inſpection, to enable us,

L'Anatomie comparée, et les expériences faites ſur les animaux vivans, nous ont devoilé une partie des fonctions vitales, naturelles, et animales. Parmi ces dernieres, celles qui dependent de la diſpoſition organique des parties et qui ſont peut-être le principe de beaucoup d'autres, pourroient recevoir une nouvelle lumiere des obſervations à faire ſur les animaux, qui ſemblent s'éloigner le plus de notre maniere d'être. Surpris par les phenomenes qu'ils nous préſentent, on fouille avec avidité dans le ſein de la nature; on y apperçoit des effets qui nous aprenent que la conformation la plus intime de certains animaux ne s'accordant pas avec nos anciennes idées, nous devons rejetter comme préjugés, des loix qu' on s'étoit trop preſſé de généraliſer. Ces effets, et la maniere de les obſerver, qui ſemblent au premier coup d'oeuil devoir nous raprocher peu à peu de la cauſe, ſoutenir nos eſpérances, flater notre

us, by degrees, to investigate the cause, support our hopes, and flatter our vanity; are nevertheless meant to shew us that the marvellous works of nature far exceed what we know, or even are able to know of them. Convinced of that truth, and in order to improve my customary walks by the sea side to some useful purpose, I bestowed a particular attention, in the month of November 1771, upon the *Sea-Anemonies*, a most singular species of animals, on which naturalists have hitherto made less ample and accurate observations. Some of those writers have even confined themselves to mere conjectures. My first success in these discoveries soon turned these amusements of mine into a long study, much more laborious than that made in a library.

There

notre vanité, n'en paroissent pas moins destinés à nous faire voir que les merveilles de la nature sont bien au dessus de ce que nous connoissons, et sans doute de ce que nous pouvons connoitre. Pénétré de ces vérités, et dans la vue de répandre une sorte d'intéreст dans les délassemens que j'ai coutume de prendre au bord de la mer, je jettai les yeux dès le mois de Novembre 1771, sur les *Anémones de Mer*, animaux singuliers sur lesquels les naturalistes n'avoient fait jusque là que des observations moins précises et moins étendues; souvent même s'en sont-ils tenus à de simples conjectures. Mes premiers succès changerent bientot ces récréations en un travail long et plus pénible que ceux du cabinet.

There is great confusion in the descriptions which naturalists have given of these animals, and no less in the names bestowed upon, and the divisions or classes assigned to, them. Some have called them *Sea-Nettle* (Urticæ Marinæ), though these animals are not prickly, as some of the wandering *Nettles* are, upon which I have, for some time past, made observations which I intend to pursue. Other writers have called them *Sea-Anemonies*. This essay, together with the plates annexed, will satisfy the reader as to the name I have preferred. It would be needless to insist longer about names.

The Sea-Anemonies, found on the coast of the Havre, seem to me to constitute three different species. Those which I put in the first class (because in certain positions they resemble most the flower known by the name of Anemone) cling or adhere to rocks and stones, and are often found in the holes that chance

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Il regne une grande confusion dans les descriptions que nous ont donné de ces animaux les naturalistes, dans les noms qu'ils leur ont imposés, et dans les divisions qu'on en a faites. Quelques uns les nomment Orties Marines *(Urticæ Marinæ)* quoiqu'ils ne piquent point comme quelques orties errantes, sur lesquelles j'ai fait depuis longtemps des observations que je me propose de continuer. D'autres leur ont donné le nom d'*Anémones de Mer*. Ce mémoire, et les figures qui l'accompagnent pourront justifier le choix que j'ai fait de ce nom. Il est inutile d'étendre plus loin cette nomenclature.

Les rivages du Havre offrent des Anémones de Mer, qui m'ont paru former trois especes. Celles que je regarde comme de la premiere (parce que dans certaines positions elles ressemblent plus parfaitement à la fleur qui porte le nom d'Anémone) s'attache

chance to be in them, and ſeem to like the ſurface of the water. The outward ſhape of the body of this animal, when it contracts itſelf, is much like a truncated cone [TAB. XVI. fig. 1.], with its baſis fixed and ſtrongly clinging to the rock. Its upper part is terminated with a hollow. This cone is often perpendicular to its baſis; ſometimes it lies in an oblique poſition to it, or the baſis ſpreads itſelf irregularly; ſo that from a round, it alters to an elliptical ſhape. Sometimes it imitates pretty exactly the incloſing out-leaves of anemonies, whilſt the limbs of the animal are not unlike the ſhag, or inner part of theſe flowers, fig. 2. At other times, it aſſumes the ſhape expreſſed by fig. 3. Indeed, theſe animals alter their forms ſo often, that it would be difficult, perhaps even impoſſible, to deſcribe them exactly. One part of their

s'attache aux rochers, aux cailloux, ſouvent même dans les troux qui s'y trouvent, et ſe plait à la ſurface de l'eau. La forme extérieure du corps de l'animal, lorſqu'il eſt retiré ſur lui même aproche beaucoup de celle d'un cone tronqué, (Pl. XVI. Fig. 1.) dont la baſe eſt apliquee et fortement atachée contre les rochers, et la partie ſupérieure ſe termine par une convexité. Souvent ce cone eſt perpendiculaire à la baſe; quelquefois il eſt à ſon égard dans une poſition oblique, la baſe s'élargit irrégulierement, en ſorte que de ronde elle devient elliptique. On la voit quelquefois imiter aſſez bien le manteau d'une anemone, tandis que les membres de l'animal paroiſſent en être la pluche, Fig. 2. d'autres fois il prend la forme de la figure 3. D'ailleurs ces animaux varient ſi ſouvent leur forme, qu'il ſeroit dificile, peut-être même impoſſible, de les bien décrire. Une partie de leur

corps

their body or limbs ſwells, at times, very conſiderably, at the expence of the reſt. The figures and the particular obſervations will ſupply what is wanting here. With regard to their colours, they vary amazingly. Every hue of purple, green, brown and violet is to be ſeen blended together. A great number of them are of one uniform colour; whilſt others are ſpotted either ſymetrically, as in ſtripes, or in an irregular, but always pleaſing, manner. Moſt of them have, round their baſis, a blue or white ſtreak, broader or narrower, which produces a ſort of ring. When many of theſe animals are put together at the bottom of a flattiſh and wide veſſel, the whole appears as a bed of Anemonies.

The Sea-Anemonies of the ſecond ſpecies are pretty nearly ſhaped out as thoſe of the firſt, but they are much larger. I have ſome by me, kept in ſea

corps ou de leurs membres s'augmente quelquefois conſidérablement aux dépends des autres. Les figures et le détail des obſervations ſupléeront à ce qui manque ici. Quant à leurs couleurs elles varient étonnenment; toutes les nuances de pourpre, de verd, de brun, de violet y ſont employées et combinées. Un grand nombre ſont partout d'une même couleur, d'autres ont des taches ſimetriquement rangées formant des rayes, ou même diſtribuées irrégulierement, mais toujours d'une maniere agréable. Preſque toutes ont autour de leur baſe une bande bleüe ou blanche, plus ou moins étroite, qui y forme une bordure. Lorſqu'on en a beaucoup d'ouvertes dans le fond d'un vaſe plat et large, il ſemble d'un parterre d'anémones.

Les Anémones de Mer de la ſeconde eſpece ſont à peu près formées comme celles de la premiere, mais elles ſont beaucoup plus grandes. J'en conſerve dans l'eau de mer, dont les membres

fea water, which are eighteen or twenty inches in circumference. Their cloak or outward fkin is rough like fhagreen, or full of little knobs. See Tab. XVI. fig. 10. and Tab. XVII. fig. 11. and 12. where they are fhewn in the natural fize. They remain in the fand, fticking to the loofe ftones in it, and ftretch out their limbs to the top, in order to lay hold of their prey, as foon as it touches the fuperficies of the fand. The flower of poppies is faid to be the plague and diftrefs of painters, to reprefent exactly the variety and brilliancy of its colours; the fame may be faid of the Sea-Anemonies of this larger fpecies. The pureft white, carmine, and ultramarine, would hardly be bright enough to paint them properly. The limbs of fome of them are of a moderate or dim colour, at the fame time that the cloak is made up of the brighteft colours.

The third fpecies feems to deviate a little more from the fecond, than this from the firft. Its body, not unlike for fhape and colour to the ftalk of a mufh-

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membres étendus forment une circonférence de 18 à 20 pouces; leur robe (la peau exterieure) eft chagrinée ou remplie de boutons; voyez la Pl. XVI. Fig. 10. & XVII. Fig. 11. 12. où elles font repréfentees de grandeur naturelle et moienne. Elles reftent dans le fable, atachees à des cailloux, et étendent leurs membres à la fuperficie pour faifir la proye qui les touche. S'il eft vrai que les têtes de pavots faffent le defefpoir des peintres par la variété et l'eclat de leurs couleurs, on pourroit en dire autant des Anémones de Mer de cette grande efpece; le beau blanc, le carmin, l'outremer, fuffiroient à peine. Quelques unes ont les membres d'une couleur douce et éteinte, tandis que la robe eft de des couleurs riches et fort hautes.

La troifieme efpece femble s'ecarter un peu plus de la feconde que celle ci de la premiere; elle eft auffi dans le fable, fon corps

mufhroom, is terminated in its lower part by a bafis, which the animal fixes to the ftones in the fand, whilft, by lengthening out its body, it affords means to the fuperior part, where the limbs and the mouth are placed, of fpreading out and opening themfelves at the furface of the fand. See fig. 4. This fpecies has fome flight variety in point of fhape, and ftill more of colour. Some have their limbs of a bright white, or fine violet colour; others of an ivory white. Some are found of the fame fort of yellow with the infide of melons. Some are greenifh, or of a fine brown, with the middle white, which gives them a likenefs to *Auriculas*. Others again have their limbs of a greyifh tint, fomewhat like the infide of a broken piece of filver; or alternately mixed with black and white in the manner of the quills of a porcupine.

I fhall now proceed to the particular experiments and difcoveries I have made hitherto upon the three fpecies.

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corps affez femblable pour la forme et pour la couleur au pédicule d'un champignon, eft terminé à la partie inférieure par une bafe que l'animal atache aux cailloux dans le fable, tandis que le corps en s'allongeant permet à la partie fupérieure, où font les membres et la bouche de s'ouvrir à la fuperficie, Fig. 4. Cette efpece a quelques légères variétés de forme et encore plus de couleur; les unes ont les membres d'un beau blanc, ou d'un beau violet, d'autres couleur d'ivoire; il s'en trouve qui les ont couleur de chair de melon jaune; plufieurs font verdâtres ou d'un beau brun avec un milieu blanc qui leur donne de la reffemblance avec la fleur qu'on nomme oreille d'ours; d'autres ont les membres gris d'argent caffé, ou mêlé alternativement de blanc et de noir, comme les piquans d'un porc épic.

Paffons au détail des expériences et des découvertes que j'ai faites jufqu'ici fur ces trois efpeces. En juftifiant ce que

ſpecies. In juſtifying what I laid down at firſt, they will convince us more and more what little progreſs has been made in the knowledge or hiſtory of *re-productions*; how far the reſources and powers of nature may extend; the uncertainty of our conjectures both upon what really conſtitutes the animal, and the exact moment that it paſſes from life to death. What an extenſive field does the ſtudy of animal œconomy, that moſt beautiful and uſeful part of phyſic, offer ſtill to our inveſtigation! At the ſame time that it encreaſes our knowledge in that branch, which is looked upon as the baſis of the art of healing, it may help us to find out whether our ſea-flowers are deſtructive to crabs and other *cruſtaceous* fiſh, to muſcles, &c. and whether they themſelves might not prove a palatable and uſeful food. Theſe reſearches might alſo afford proofs that the ſtructure of thoſe animals which are looked upon as little deſerving of our attention, are ſtill, ſome

que j'ai établi au commencement, elles pourront faire entrevoir combien nous ſommes peu avancés dans l'hiſtoire des reproductions; quelles peuvent être les reſſources et les forces de la nature; l'incertitude de nos conjectures ſur ce qui conſtitue l'animal, et ſur le moment précis où il paſſe de l'état de vie à l'etat de mort. Quel vaſte champ nous offre encore à cultiver l'etude de l'œconomie animale! cet objet le plus beau et le plus utile que la phyſique nous préſente, eſt propre en même temps à augmenter la ſomme de nos connoiſſances dans cette partie qu'on regarde avec raiſon comme la baſe de l'art de guerir; à determiner ſi ces animaux detruiſent les cancres et autres cruſtacés, les moules, &c. ou s'ils ne pourroient pas devenir eux memes un mêts recherché et delicat; à nous convaincre que la ſtructure des animaux qu'on juge peu dignes d'attention,

ſome by their complicate make, and others by their very ſimplicity, more incomprehenſible than larger and better-known animals. It may in ſhort keep up our admiration, by obſerving how the omnipotence of the Creator ſhines forth even in creatures, which, like theſe, ſeem produced to be trampled upon, or at beſt to be perceived by chance.

What firſt offered itſelf to my obſervations, is what diſtinguiſhes theſe animals from plants, I mean progreſſive motion, by the help of which they can ſhift their ſpot; the other determinate motions, by which they are enabled to lay hold of their prey; the means they make uſe of to defend themſelves; their deglutition, digeſtion, evacuations, and laſtly the propagation of their ſpecies, &c. What little I have had an opportunity to ſee of thoſe functions, appears to me ſufficient to place theſe creatures in the claſs of ſpontaneous animals, rather than in the dark indeterminate liſt of *zoophytes*.

In

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d'attention, offre dans les uns par ſa complication, dans les autres par ſa ſimplicité, quelque choſe de plus incompréhenſible que celle des animaux plus grands et plus connus; enfin à ſoutenir notre admiration en nous faiſant obſerver combien la grandeur de Dieu éclate juſque dans les créatures qui ſemblent comme celles-ci deſtinées à être foulées aux pieds, ou à n'être apercües que par hazard.

Ce que j'ai du d'abord obſerver, c'eſt ce qui diſtingue ces animaux des plantes, comme le mouvement progreſſif à l'aide duquel ils changent de place, les autres mouvemens déterminés par leſquels ils ſaiſiſſent la proye, les moyens qu'ils employent pour ſe défendre, la déglutition, la digeſtion, la déjection, la propagation de l'eſpece, &c. le peu que j'en ai vu m'a paru ſuffiſant pour leur aſſigner une place dans le regne animal, et pour les tirer du genre obſcur et indéterminé des zoophites.

Au

In May 1772, I clipped all the limbs of a purple Anemone of the firſt ſpecies. Soon after, theſe limbs began to bud out again. The 30th of July, they were clipped a ſecond time, and grew again in leſs than a month. Having cut them a third time, they had a third ſhooting out. The ſame experiment upon a green Anemone had the like ſucceſs. It ſeems theſe reproductions might extend as far, or be as often repeated as patience and curioſity would admit. Several experiments have convinced me that one ſingle limb of theſe Anemonies being cut off, retains a power to faſten itſelf to any ſmall body that is brought near it, either by its end, or by the ſide towards the end, but not by that part where the clipping was made. This induces me to think that the effect is produced by ſuction rather than by any glutinous matter, which one might ſuppoſe ouzes out at the pores. This limb, after being cut off, has alſo a power to ſtretch or contract itſelf alternately.

July

Au mois de Mai 1772, je coupai tous les membres à une anemone pourpre de la premiere eſpece. En peu de temps ces membres repoulſerent. Le 30 Juillet ils furent coupés de nouveau, et ſe reproduiſirent en moins d'un mois. Ayant été coupés une troiſieme fois ils ont encore repouſſé. Une anemone verte a donné le même réſultat. Il paroit que ces reproductions iroient auſſi loin qu'on auroit la curioſité ou la patience de les pouſſer. Pluſieurs expériences m'ont appris que juſqu'à un ſeul membre retranché de ces animaux, conſerve pendant pluſieurs jours la faculté de s'attacher aux corps qu'on lui preſente, ſoit par le bout, ou par le coté vers le bout, et non par la partie coupée; ce qui m'a porte à croire que cet effet pouvoit être produit par la ſuction plutôt que par la glu qu'on ſoupçonneroit exuder des pores. Ce membre coupé ſe contracte et ſe dilate auſſi alternativement.

 Le

July the 12th I cut one of theſe purple Anemonies through the body, rather nearer the baſis. This part remained adhering to the ſide of the veſſel in which it was; and for ſeveral days, made various motions. At laſt, it got looſe, and then faſtened in another place. The 27th it began again to move about, till the end of Auguſt, when it became as it were lifeleſs, grew very flabby, and had often an offenſive ſmell. I concluded it to be dead; but, as it did not loſe its ſhape, I reſolved to keep it, and to ſhift it every day into ſome freſh ſea-water. From time to time, I thought it had ſome ſort of motion, and in the beginning of November theſe motions became more perceptible. It ſhifted its poſition, when contrary to its natural ſtate. November the 28th, this *ſtump* climbed up to the top of the veſſel. I then began to perceive ſome new limbs growing out. January 13,

14,

Le 12 Juillet je coupai par la moitié du corps et même plus proche de la baſe une anemone pourpre. Cette baſe reſta attachée aux parois du vaſe, et fit pendant pluſieurs jours différentes marches, après leſquelles elle ſe détacha, et s'attacha de nouveau. Le 27 elle recommenca ſes courſes juſqu'à la fin d'Aouſt, où elle ceſſa de donner ſenſiblement des ſignes de vie, devint d'une molleſſe extrême, et répandit même à pluſieurs repriſes une mauvaiſe odeur: je la jugeois morte; cependant comme elle ne ſe déformoit point, je pris la réſolution de la garder, et de lui procurer tous les jours de nouvelle eau de mer. Je crus appercevoir de loin à loin quelques mouvemens. Au commencement de Novembre, ſes mouvemens devinrent plus ſenſibles, et juſqu'à ſe retourner, quand elle étoit dans une poſition contraire à celle qui lui eſt naturelle. Le 28 Novembre elle grimpa au haut du vaſe, alors je crus appercevoir des membres renaiſſans. Les 13, 14, et 15 Janvier, elle a marché de

nouveau,

14, and 15, 1773, it again walked about: and on the 16th, feeing thefe growing new limbs, I offered them fome bits of mufcles, which however were neither eat, nor even laid hold of. That fame day, after feveral motions in various directions, it loofened its adhefion, and remained motionlefs and flabby, but without any bad fmell, till the beginning of February, when it appeared adhering, but weakly, to the bottom of the vafe. The 16th, after feveral motions, it climbed up to the top, where it remained till the 11th of March, and then loofened its hold. Thefe alternate ftations and motions lafted till the 8th of April, without my feeing any plain and full re-production. However the animal continues to grow ftronger and thicker. I own I was very wrong to throw away, a few days after the operation, the upper part that had been cut off. But I did not forefee what would happen.

November

nouveau. Le 16 appercevant des apparences de membres je leur préfentai de petits morceaux de moule, qu'elle n'a ni mangés ni retenus. Ce meme jour, après plufieurs mouvemens en divers fens, elle s'eft détachée et a refté ainfi molle et immobile jufqu'au commencement de Fevrier, fans pourtant porter de mauvaife odeur; enfuit eelle a paru foiblement attachée au fond du vafe; elle a monté au haut le 16, après plufieurs mouvemens, où elle a refté jufqu'au 11 Mars, qu'elle fe détacha. Ces alternatives et ces mouvemens ont duré jufqu'aujourd-hui 8 Avril, fans que je voye paroitre de reproduction réelle; cependant elle continue de fe fortifier et devient de plus en plus épaiffe. Je jettai fans doute fort mal à propos dès les premiers jours la partie retranchée; pouvois-je prévoir ce qui devoit arriver.

Le

November the 9th, 1772, I clipped a brown Anemone through the body. The baſis, together with that part of the ſtump which was left to it, ſhrunk up, as figure 5. ſhews, and remained motionleſs where it was at firſt, till January 13, when it ſhifted its place. The 15th, I very diſtinctly perceived two rows of limbs growing out of the part where the ſection was made, fig. 6. and the animal moved along. The next day I offered it bits of muſcles, which it laid hold of and ate. Theſe growing limbs were, at firſt, of a ſullied white, they grew browner and browner every day, and are at preſent of the ſame colour with the coat of the animal. They are pretty near as large as they were before the operation; but I have not perceived, as yet, ſome of thoſe ſmall fine blue knobs, that are to be ſeen round the rim or upper ſkurt of the coat, as may be ſeen in figure 2. As

To

Le 9 Novembre je coupai par la moitié du corps une anemone brune; la baſe avec la partie du tronc qui lui reſtoit ſe retira ſur elle même, comme la repréſente la figure 5; elle reſta immobile au lieu où elle étoit attachée, juſqu'au 13 Janvier qu'elle marcha. Le 15 j'apperçus très diſtinctement deux rangs de membres renaiſſans de la ſection même, figure 6, et l'animal marchoit. Le lendemain je lui préſentai des morceaux de moule, il les ſaiſit et les mangea. Dabord ces membres renaiſſans étoient d'un blanc ſale; mais peu à peu ils ont bruni et approchent maintenant de la couleur de la robe de l'animal. Il s'en faut très peu qu'ils n'ayent la même grandeur qu'ils avoient auparavant; je n'ai cependant point encore vû paroître certains petits globes d'un beau bleu engagés dans le tour du [illegible] au bord ſupérieur de la robe, comme on peut voir par la figure 2, manquant à la partie retranchée, repréſentée

par

to the part cut off, ſeen in figure 7, which conſiſts of about half the body, and wherein the limbs and mouth are placed, I offered it, after the operation was performed, that brown part of a muſcle, by the help of which it moves along, and whence the beard ſpreads out. This bit, which is not eaſily digeſted by Sea-Anemonies, was directly ſnapped up by the limbs. They drew it to the mouth, which lengthened itſelf out to catch it, and ſwallowed it down. But, as the body was wanting to receive it, the bit came out at the oppoſite end, juſt as a man's head, being cut off, would let out, at the neck, the bit taken in at the mouth. I offered it a ſecond time, and the animal ſwallowed it again; but threw it up at the mouth the next day. I ſtill keep that part of the Anemone, which daily grows ſtronger and ſtronger, and which appears to me to ſuck in the bits of muſcles I offer it. The limbs lay hold of them, and the mouth takes them in, either whole or in part, and throws them up

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par la fig. 7. qui comprend environ la moitié du corps, où ſe trouvent les membres et la bouche, je lui préſentai peu après l'operation un morceau de moule où étoit ce membre brun qui lui ſert à ſe traîner, et où eſt attaché ſa filiere, morceau que les anemones de mer ont peine à digérer; les membres le ſaiſirent, l'approcherent de la bouche, la bouche même s'allongea pour le recevoir, et il fut avalé: mais comme il n'y avoit point de corps entier pour le recevoir, il ſortit par l'autre extrémité, comme une tête tranchée qui avaleroit pour le rendre par le col. Je la lui preſentai de nouveau, elle l'avala une ſeconde fois et la rendit par la bouche le lendemain. Je garde encore cette partie qui ſe fortifie chaque jour et me paroît ſucer les morceaux de moule que je lui préſente; les membres les embraſſent, la bouche les reçoit en tout ou en partie, et me les rend fort altérés, ſouvent

up a good deal altered; and pretty often these bits go through as they did the first time. Some persons, who were eye-witnesses of these particulars, were of opinion that, from the remains of organization and habit, this part of the animal still endeavoured to gratify a natural want, though no longer subsisting; but I am inclined to think that it still exists. In my opinion the part is nourished by means of suckers, of which I suspect it to be full, both inwardly and outwardly. I am in expectation to have my conjecture confirmed, by experience, and by it to be enabled to convince other people. The microscope seems to have already corroborated my notion upon this subject. When the limbs of these Anemonies, especially those of the *second* species, are touched, the person's fingers are felt to adhere and strongly to stick to the limbs. I have therefore let both these, and several other species of these animals, fasten several times upon the fingers of one of my hands, in order to see whether any glutinous matter should remain upon them; but I never

souvent ils passent d'outre en outre comme la premiere fois. Ceux qui en ont été témoins pensoient que, par un reste d'organization et d'habitude, cette partie de l'animal cherchoit encore à satisfaire un besoin qui n'existoit plus; pour moi je suis porté à croire que ce besoin existe, que la partie peut se nourrir par des sucoirs dont je soupçonne qu'elle est remplie intérieurement et extérieurement, et j'attens de l'experience ce qui me manque pour le prouver aux autres. Le microscope m'a déja paru fortifier mon sentiment; lorsqu'on touche les membres des anemones entr' autres de la seconde espece, on sent ses doigts fortement attachés à ces membres. Je les ai donc laissés s'attacher à plusieurs reprises et sur plusieurs anemones pour voir s'il ne resteroit pas à mes doigts quelque sorte de glu, et je n'y en ai jamais

never perceived any; and by applying the fingers of that hand to the other, I perceived no adhesion.

I have, for some days past, clipped several Anemonies of the first species, diametrically and perpendicularly to the basis. They have stood the operation extremely well. Time will teach us what the result of these operations will be.

The 5th of February last, I placed a glass vessel, exposed to the morning frost, with two Anemonies, the one brown the other purple, which stuck to the sides of it towards the top. The surface of the water froze, and remained frozen the whole day: in the night-time, the thickness of the ice increased to such a degree, that there remained but a little water not frozen in the bottom of the glass. The Anemonies were thus inclosed in a pretty strong piece of ice, except the basis, which remained adhering to the bottom. The glass was next day placed near the fire, in order to melt the ice by degrees, and when

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jamais apperçu: j'ai porté ces doigts sur mon autre main, et il ne s'en est suivi aucune adhérence.

J'ai coupé depuis plusieurs jours des anemones de la premiere espece diamétralement et perpendiculairement à leur base, elles ont soutenu l'operation fort bien; le temps nous en devoilera les résultats.

Le 5 Fevrier dernier au matin j'exposai à la gelée un vase de verre contenant deux anémones, l'une brune, l'autre pourpre, attachées aux parois vers le haut du vase; la surface de l'eau se glaça, et resta en cet état toute la journée; pendant la nuit l'épaisseur de la glace augmenta tellement qu'il ne restoit qu'un peu d'eau dans le fond, de sorte que de tous cotés excepté la base adhérente au verre, mes anemones, étoient enfermées dans un fort glaçon; le lendemain je mis le verre devant le feu pour faire fondre peu à peu la glace. Quand elle

when it began to float, I ſaw plainly the Anemonies were quite detached from the ſides, becauſe their body turned round looſe, with the ice, though their baſis had remained exactly in the ſame ſituation they were in before. Mr. *Reaumur's* thermometer being in my room, at two degrees above the freezing point, or o, theſe animals gave ſome ſign of life; and the 8th, the thermometer being at 3 ½ they opened; while the Anemonies, which were in other glaſſes, and had not been expoſed to a greater cold than that of one degree above froſt, did not open yet; and I generally obſerved they only opened at 5° of the thermometer. May it in conſequence be ſuppoſed that thoſe Anemonies, which had been frozen in the ice, were leſs affected with a cold at 3 ½ than the others?

March

elle devint flotante, j'eus lieu d'apercevoir que les anemones avoient ceſſe d'être attachées aux parois du vaſe par leur baſe, parcequ'elles tournoïent avec la glace, quoique leur baſe eut abſolument demeuré dans le même etat où elles étoient auparavant. Le thermometre de Mr. de Reaumur étant dans mon cabinet à deux degrés de dilatation, ces animaux donnerent quelques ſignes de vie, et le huit le thermometre étant à 3 ½ s'ouvrirent tandis que celles des autres vaſes qui n'avoient point éprouvé de froid plus grand qu'un degré de dilatation ne s'ouvroient pas encore. Car j'ai remarqué qu'elles ne s'ouvrent, à parler généralement, que lorſque le thermometre parvient à cinq degrés. Celles qui ont éte enfermees dans la glace auroient-elles par comparaiſon trouvé la temperature de trois degrés et demi plus douce que ne la trouvoient les autres?

Le

March the 19th, I ſet upon the fire a glaſs veſſel, with the two animals which I had made uſe of for the foregoing experiments. A thermometer put into the ſame glaſs was at 8°; when it came to 15 and 17, the Anemonies opened themſelves a little; but, at 28 and 30, they appeared to be in pain; at 32 they cloſed themſelves up; at 37 one of the two fell off from the ſides; upon which I poured in ſome freſh water, and took the glaſs from the fire. Soon after they opened, and one of them produced two young ones the next day. In another experiment, the ſame Anemone which dropped off at 37 degrees, detached itſelf at 26, and cloſed up at 28. In a third experiment I ſhifted, at once, one of theſe Anemonies from 8° to 40°, and left it to remain in that heat for five minutes; it appeared to be in pain for ſeveral days after, its coat was even affected on one ſide, but, at

Le 19 Mars je plaçai ſur un feu doux un vaſe de verre, contenant les deux anemones qui m'avoient ſervi aux expériences précédentes, avec un thermometre plongé dans le même vaſe; il étoit à 8 degrés de dilatation, lorſqu'il eſt parvenu à 15 et 17; les anemones ſe ſont un peu ouvertes; mais à 28 et 30, elles ont paru ſouffrir; à 32 elles ſe ſont fermées; à 37 une des deux s'eſt détachée des parois; j'ai alors promptement remis de nouvelle eau, et retiré le vaſe du feu, peu après elles ſe ſont ouvertes et une a donné deux petits le lendemain. Dans une autre expérience, la même anemone qui s'étoit détachée à 37 dégrés, s'eſt detachée à 26, et s'eſt fermée à 28. Dans une troiſieme expérience, j'ai fait paſſer ſubitement une de ces anemones de 8 degrés de dilatation à 40; je l'y ai laiſſée pendant cinq minutes; elle a paru ſouffrir pendant pluſieurs jours, ſa robe a même été entamée par un coté; mais enfin elle

at laſt it recovered its ſtrength, clung to the ſides, climbed up to the top of the glaſs; and all was well again. I have ſhifted another Anemone at once from 8° to 40°: but, inſtead of leaving it only five minutes, I went on increaſing the heat to 46°, in order to obſerve the effect. The coat appeared to me to ſwell out a little; at 50° it ſtill encreaſed, and the animal died in conſequence.

I put ſeveral Anemonies, ſome in, and ſome without, water, under the recipient of an air-pump; and got the quickſilver of the barometer incloſed in it, to come down within an inch of the level. The animals did not ſeem to perceive the effects of a *vacuum*, or an air ſo much rarefied. They remained open, or in whatever ſtate they happened to be when put in, and did not ſwell out. When the air was let in again, and they were not in the water, I perceived no ſhrinking at all in any of them.

These

elle a repris vigueur, s'eſt attachée, et a gagné le haut du vaſe; il n'y paroit plus. Du même point de 8 degrés j'ai fait paſſer ſubitement une autre anemone à 40, et au lieu de ne l'y laiſſer que cinq minutes, j'ai obſervé ce qui ſe paſſeroit en continuant de chauffer; à 46 degrés la robe m'a paru ſe bourſoufler un peu; à 50 degrés elle l'etoit encore plus, de ſorte que l'animal en eſt mort.

J'ai mis dans l'eau, et ſans eau, ſous le récipient d'une machine pneumatique pluſieurs anemones, j'ai abaiſſé le mercure du barometre qui y étoit renfermé, juſqu'à un pouce de ſon niveau. Elles n'ont pas paru s'appercevoir de l'effet du vuide, ou d'un air ſi fort raréfie: elles ont reſté ouvertes, ou comme elles étoient, ne ſe ſont point gonflées, et lorſque j'ai rendu l'air au récipient, et qu'elles n'étoient point dans l'eau je n'ai apperçu en elles aucun affaiſſement.

Il

These animals can live a whole twelvemonth, and perhaps much longer, without any other food but what they chance to find disseminated in the sea-water. They do not want many motions to procure their food, besides stretching out their limbs, to receive such as comes within their reach; and they remain surrounded with muscles, &c. without laying hold of any of them. I have given Anemonies some of these muscles alive, but with their shells closed, and about six lines in length. They were swallowed in that state; and 40, 50, and 60 hours after, the shells were thrown up at the mouth empty and perfectly cleared, even from the small tendons which connect the fish to its shells. The Anemonies swallow and digest small fish, and bits of larger fish, or of raw meat, when offered to them. When they cannot digest some of the food, they throw it up at the mouth, either whole or partly dissolved into

Il paroit que ces animaux peuvent vivre pendant une année, et peut-être beaucoup plus, sans autre nouriture que celle qu'ils trouvent disséminée dans l'eau de la mer; qu'ils ne font pas beaucoup d'autres mouvemens pour s'en procurer d'autre que celui d'étendre leurs membres pour recevoir celle qui vient d'elle même y toucher: car ils restent environnés de moules, &c. sans s'en emparer. Je leur en ai donné de 6 lignes de longueur vivantes et fermées, les anemones les ont avalées en cet état, et 40, 50, ou 60 heures après, elles m'ont rendu par la bouche les deux coquilles vuides et parfaitement nettoyées, même des petits tendons qui attachent le poisson aux deux coquilles. Elles avalent et digerent des petits poissons, et des morceaux de poisson ou de viande fraiche que je leur présente. Lorsqu'elles ne peuvent digérer quelque chose, elles le rendent

into a viſcous liquor, which may in ſome meaſure be looked upon as their excrements. They void by the ſame way, and in the ſame manner, various parts of a white and brown ſubſtance, and ſmall bodies are thrown out at the extremity of the limbs, and more ſenſibly in the ſecond ſpecies. There alſo ouzes out of their body a ſort of excrementitious matter, which by coagulating produces round them a ſort of girdle.

Theſe animals are known to be *viviparous*. Several of them have brought forth, even in my hand, 8, 10, and 12 young ones. Some, though almoſt imperceptible, as in fig. 8, have nevertheleſs the power of clinging, and are endowed with two rows of limbs, which they open immediately upon their birth, in order to catch their prey, which they ſwallow afterwards. I have kept ſome for ten months. They have appeared to me not to have increaſed in their bulk, more than twice

dent par la bouche ou en entier ou réduit en partie en une eſpece de glaire qu'on peut regarder comme leurs excrémens, quoiqu'on leur voie rendre auſſi par la même voye différentes parties de matiere blanche ou brune, et de petits corps par le bout de leurs membres, ce qui eſt plus ſenſible dans la ſeconde eſpece. Il ſort auſi de leur corps une ſorte d'humeur excrémentitielle, qui forme tout autour en ſe coagulant comme une eſpece de ceinture.

Ou ſait que ces animaux ſont vivipares; pluſieurs m'ont donné, même dans ma main 8, 10 et 12 petits, les uns preſqu'imperceptibles, figure 8, n'en ont pas moins la faculté de s'attacher, et ont un ou deux rangs de membres qu'ils ouvrent auſſitôt après leur naiſſance pour ſaiſir la proye qu'ils avalent enſuite; j'en ai gardé pendant dix mois; ils ne m'ont paru augmenter en groſſeur que du double de leur diamètre. Il eſt

twice the diameter of their ſize. Indeed I fed them ſparingly; the others are grown as big as half a green pea. Fig. 9.

The Sea-Anemonies have a progreſſive motion; which, though ſlow, is performed in every direction, with a degree of facility, and effected by means of muſcles, which croſs one another at right angles. I cannot as yet diſplay the mechaniſm of theſe muſcles, becauſe the laſt mutilations I was obliged to make diſcompoſe the conjectures that ſome men of learning have publiſhed upon this ſubject; and I have not ſufficiently fixed my thoughts in conſequence of my obſervations.

Theſe animals, being put into freſh water, die very ſoon after, and whatever their colour be when they are put in, they ſoon turn pale, and remain pretty near in the ſame poſition that they were in. Soon after, their coat becomes ſo flabby, that it can very

vrai que je leur ai donné peu de nouriture; les autres ſont gros comme la moitié d'un pois verd, Fig. 9.

Les anemones de mer ont un mouvement progreſſif, à la verité aſſez lent, mais qui s'exécute pourtant en tous ſens avec une ſorte de facilité. Il dépend de muſcles qui ſe croiſent à angles droits; mais je ne puis en déveloper ici le méchaniſme, parceque les dernieres mutilations que j'ai faites déconcertent les conjectures que quelques ſavans avoient publiées ſur ce point, et qu'en conſéquence je n'ai pu encore fixer mes idées à ce ſujet.

J'ai mis des anemones de mer dans de l'eau donce, elles y ont péri preſqu'auſſitôt, on les voit palir un peu, de quelque couleur qu'elles ſoient et demeurer à peu près dans la même poſition qu'elles étoient au moment qu'on les y a plongées. Peu de temps après, leur robe devient ſi facile à entamer, qu'un pein-

very eaſily be ſtripped off in ſhreds. For this reaſon, when I boiled them, I always made uſe of ſea-water.

I have not been able hitherto to perceive any eyes in theſe creatures; and yet they ſeem to be affected by the light. I have had opportunities to ſatisfy myſelf on this ſubject at every hour of the night. For by holding up a lighted candle over glaſſes, which contained a great number of them, but at ſuch a diſtance as not to communicate any heat, I could ſee them, either by degrees, or even ſuddenly, cloſe up, and not ſpread out again 'till a while after the light was removed. The ſame effect was produced upon repeated trials. This, however, admits of ſome exceptions, viz. when they had fed plentifully, they were ſlower in ſhutting themſelves up, and even ſometimes did not cloſe up at all. The brighter the light let in upon them

pinceau l'enleve aiſément par lambeaux; c'eſt pourquoi quand j'en ai fait cuire, j'ai employé de l'eau de mer.

Juſqu'ici les anemones ne m'ont point paru avoir d'yeux, elles ſont pourtant affectées par la lumiere. J'ai eu occaſion de le remarquer à toute heure de la nuit; car en tenant au deſſus des vaſes, qui en contenoient un grand nombre, une bougie allumée de laquelle elles ne pouvoient reſſentir aucune chaleur, je les voyois peu à peu quelquefois même ſubitement ſe fermer, et elles ne s'ouvroient de nouveau que lorſque je faiſois diſparoitre la lumiere pour quelque temps, après lequel ſi je la leur préſentois, elles ſe fermoient comme la premiere fois. Ceci a quelques exceptions qui ne détruiſent point la loi générale; par exemple, lorſqu'elles ont beaucoup mangé elles ſe ferment moins vite et même point du tout. Plus la lumiere eſt vive en proportion de celle dont elles jouiſſoient au

them is, in proportion to the degree of that which they were in before, the more affected do they appear to be by it. Is then the texture of the body of thofe creatures fuch, as not, indeed, to receive the impreffions of objects, with the fame degree of perfection as our eyes do; but, to afford a general organ affected by light, as the bodies of other animals are by feeling?

Might not thefe animals become a frefh fupply for our tables; and thereby make amends for the confumption of mufcles, on which they feed? In order to try the experiment, I gave fome Anemonies, boiled in plain water, to a cat, who devoured readily twenty of them, feveral of which were of the third fpecies, without any bad effect, and feemed wifhful to have more. This induced me to make a further trial, and to get fome of them colloped as we do oyfters in

auparavant, plus elles y paroiffent fenfibles. Le corps de ces animaux feroit-il donc, je ne dis pas un organe capable de recevoir l'impreffion des images, avec les conditions néceffaires à une fenfation auffi parfaite que la procure notre oeuil, mais une efpece d'organe général fenfible au contact de la lumiere comme le toucher des autres animaux l'eft à celui des autres corps?

Ces animaux ne pourroient-ils point devenir une nouvelle reffource pour nos tables, et réparer ainfi la perte des moules dont ils fe nourriffent? pour m'en affurer, j'en fis donner de bouillis, fans aucun affaifonnement, à un chat, qui en mangea avec avidité vingt, dont plufieurs étoient de la troifieme efpece; il n'en parut reffentir aucune incommodité, on jugea même à fon air qu'il en auroit mangé d'avantage. Cela me fit defirer d'avoir fur cet objet effentiel une plus grande certitude, j'en fis préparer comme on fait les huitres dans ces grands peignes, qu'on nomme coquilles de pélerin, et les ai mangé

in ſhells, I ſwallowed them up myſelf. Perhaps they may prove ſtill more palatable, dreſſed in ſome other manner. However, they may be eat ſafely.

The ſecond ſpecies of Sea-Anemonies keeps itſelf hid more than the firſt; it is not to be come at but in neap-tides, when the ſea recedes fartheſt, and cannot be ſo eaſily obſerved. With great difficulty are theſe Anemonies looſened from the ſtones they adhere to; part of the baſis is often left behind, and they are not eaſily preſerved at home. Seeing however ſome of the individuals that voided muſcle-ſhells whole, and ſtill joined together, but empty, I found out the way to feed them. Crab-ſhells, about the ſize of a hen's egg, are likewiſe diſcharged whole; and upon offering them ſome live-ones, I found that they ſwallowed them down, and voided the remains ſucked dry in about twenty hours. I cut open ſome of this ſecond ſpecies, which I could not looſen from the ſtones, and among them I found one

mangés; peutêtre une autre préparation leur conviendroit-elle mieux, toujours eſt-il certain qu'on en peut manger.

La ſeconde eſpece d'anemones de mer plus cachée que la premiere, et qu'on ne trouve que dans les grandes marées, où la mer ſe retire plus loin, eſt auſſi plus difficile à obſerver; on a peine à la détacher des cailloux ſans y laiſſer une partie de ſa baſe, et à la conſerver chez ſoi. C'eſt en obſervant quelques individus de cette eſpece et en appercevant qu'ils rendoient des coquilles de moules entieres, jointes enſemble et vuides, que j'ai appris de quelle maniere je devois nourrir mes anemones. Je leur ai vu même rendre des cancres entiers grands comme la circonference d'un oeuf de poule, je leur en préſentai de vivans qu'elles avalerent et qu'elles m'ont rendus morts, et ſucés environ vingt heures après. J'en ai ouvert que je ne pouvois détacher des cailloux, parmi leſquelles j'en trouvai

one that had ſwallowed an Anemone of the *third* ſpecies; but this had received no harm. For, having put it into ſome ſea-water, it opened and ſpread as uſual. I have offered them ſeveral of the ſame ſpecies, which they ſwallowed down; but threw up again alive within eight, ten, or twelve hours, or even later. Is then a live Anemone an undigeſtable body for another?

On the 21ſt of June, 1772, having *nicked* the inſtant, that an individual of the third ſpecies was ſtretching out, as expreſſed in Fig. 13, I ſnipped off at once with ſharp ſciſſars, the whole upper part where the limbs and mouth are placed. It was with great ſatisfaction that, eight days after, I perceived new limbs growing out, as in Fig. 15. The third of July, the animal began to eat ſome bits of muſcles; and towards the middle of the month, the upper part was ſo completely formed, that it might eaſily

trouvai une qui avoit avalé une anemone de la troiſieme eſpece qui n'en avoit ſouffert aucune altération: car l'ayant miſe dans l'eau de mer, elle s'ouvrit comme à l'ordinaire. Je leur en ai préſenté pluſieurs, elles les ont rejettees en vie au bout de huit dix ou douze heures, et même plus tard. Une anemone en vie ſeroit-elle donc un corps indigeſte pour une autre anemone?

Le 21 Juin ayant ſaiſi l'inſtant où une des anemones de la troiſieme eſpece étoit allongée, comme dans la figure 13, j'en retranchai ſubitement, avec de bons ciſeaux, toute la partie ſupérieure où ſont les membres et la bouche. Avec quelle ſatisfaction n'ai-je pas vu au bout de huit jours de nouveaux membres renaiſſans figure 15. Le trois Juillet l'anemone commença à manger des morceaux de moule, et vers la moitié du même mois, la partie ſupérieure étoit ſi bien reproduite, qu'on

easily have been mistaken for one of its unclipped neighbours, had there been many in the glass. It is neither the row of the centrical or inner limbs, nor the most outward, which first bud out, but the intermediate ones. The part, which had been clipped off, gave signs of sensibility to the 17th of July, contracting and dilating itself, in the same manner as a whole Anemone; but it was much smaller than before the operation. This experiment has been repeated by clipping, on the 11th of July, the whole upper part and one third of the body of another Anemone. New limbs began to shoot out the 21st. There were two rows of them the 25th; and on the 3d of August, four very distinct and well shaped, which caught and kept fast the food that was offered the animal. The mouth itself was sufficiently well formed to take in several times bits of muscles.

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qu'on auroit aisément confondu l'anemone avec ses voisines, si elles eussent été en grande quantité dans le même vase. Ce ne sont ni les rangs des membres les plus voisins du centre, ni ceux qui en sont les plus eloignés qui paroissent les premiers dans la reproduction, mais les intermédiaires. La partie qui avoit eté retranchée a donné jusqu'au dixsept Juillet des marques de sensibilité, se contractant et se dilatant de la même maniere que le fait l'anemone: mais elle étoit beaucoup plus petite qu'avant d'être retranchée. Cette expérience singuliere a été repétée en coupant le onze Juillet toute la partie supérieure et un tiers de la longueur du corps d'une autre anemone. Il reparut de nouveaux membres le vingt-un; le vingt-cinq elle en avoit deux rangs, le trois Aoust quatre bien formés et qui retenoient les corps qu'on lui présentoit, lorsqu'ils étoient propres à la nourriture: la bouche même commençoit à être assez bien formée pour que l'animal ait mangé plusieurs fois de

mufcles. On the 11th, I perceived in the limbs the faint alternate marks of ivory white and black, and foon after, there fcarcely appeared any fign of the operation having been performed.

Being induced to try further experiments, on the 7th of Auguft, I clipped an Anemone acrofs the body. Like the others, it moved or wriggled a little at firft: but, I did not perceive any new limbs growing till towards the end of the month. During that time, it continued in fuch a ftate as gave but little hopes of its doing well again. Two rows of limbs appeared at laft, and the infect recovered its ftrength. There was on the 9th of September a third row of limbs, and the mouth appeared to be fhaped out and formed; yet the Anemone neither ate nor kept the bits of mufcles I gave it. A fourth row was growing out on the 19th, which gathered ftrength by degrees; fo that on the

des morceaux de moule. Le onze j'aperçus dans les membres les petites alternatives de blanc d'ivoire, et de noir; et peu après il étoit difficile de s'appercevoir qu'elle eut fouffert quelque alteration.

Tenté de pouffer la chofe plus loin, le fept Aouft je coupai par la moitié du corps une anemone. Elle fe donna d'abord comme les autres quelques mouvemens, mais ce ne fut que vers la fin du mois que j'obfervai de nouveaux membres; pendant cet intervale elle fut dans un état qui laiffoit peu d'efpoir, enfin deux rangs de membres parurent et l'animal reprit vigueur. Le neuf Septembre il y avoit un troifième rang de membres, et la bouche paroiffoit formée; l'anemone cependant ne mangeoit ni ne retenoit les morceaux de moule qui lui étoient préfentés: le dixneuf on aperçut un quatrieme rang de membres, qui peu à peu fe fortifia, de maniere que le trois Octobre

the 3d of October it began to eat, and in a ſhort time became a compleat animal. On September the 22d, the upper part appeared to me to be withering away. But we ſhall ſoon ſee how much I have reaſon to think I was miſtaken.

I cut another Anemone, of the ſame ſpecies, acroſs the middle of the body, in ſuch a manner as that the two parts were only left hanging together by one fourth part of the diameter. My deſign was, to try whether nature would produce limbs on the edge of the lower half-part, in the ſame manner as when the body is cut quite aſunder; or, whether the wound, though very deep, would heal up again. Nature was not wanting to itſelf; for, notwithſtanding the largeneſs of the inciſion, the two ſevered parts were joined up together, and in a few days the wound was healed

Octobre l'anemone mangea et devint en peu de temps un animal complet. Le partie ſupérieure me parut périr le vingt deux Septembre; mais on verra combien j'ai lieu de croire que je me ſuis trompé.

J'ai encore coupé par la moitié du corps une anemone de cette eſpece, de maniere que les deux parties ne tenoient plus l'une à l'autre que par un quart du diametre du corps de l'animal. J'avois envie de voir ſi la Nature ne feroit pas naitre des membres au coté coupé de la partie inférieure, comme lorſque la ſupérieure en eſt totalement ſéparée, ou ſi la playe quoique fort grande ſe conſolideroit. La Nature ne s'eſt point trompée; malgré la grandeur de l'inciſion les deux parties ſe ſont rapprochées, et au bout de quelques jours la playe

healed up. The animal did not even ſeem to have ſuffered ſo much as one might have been apt to expect.

I have been witneſs to a fact remarkable enough to be inſerted here. Having ſliced a bit of fiſh, I offered one end of it to an Anemone, whoſe limbs are of the melon-yellow colour, and the other end to a grey Anemone, whoſe ſuperior part had been re-produced after it had been cut off. The two inſects, which were adhering pretty near one another at the bottom of the glaſs, directly ſeized their prey with their arms; but the yellow one happened to lay hold of the larger ſhare of the ſlice. Each ſwallowed on, by the reſpective ends, till at laſt each other's mouth came within contact. The grey one ſeemed, at firſt, to get the better; but the other ſoon recovered her ſhare, loſt it again, and again recovered it. Theſe alternate victories laſted about three hours; and

playe étoit conſolidée. L'animal n'en a pas même paru ſouffrir autant qu'on l'auroit imaginé.

Jai été témoin d'un fait aſſez ſingulier pour qu'il trouve place ici. Ayant coupé un morceau de poiſſon en forme d'aiguillette, je le préſentai par un bout à une anemone, dont les membres ſont couleur de chair de melon, et de l'autre bout à une anemone griſe, dont la partie ſupérieure s'étoit reproduite après avoir été coupée. Mes deux anemones attachées au fond du vaſe aſſez près l'une de l'autre ſaiſirent la proye avec leurs membres; mais l'anemone jaune eut lieu d'en embraſſer une plus grande partie. L'une et l'autre avalerent le bout qu'elles tenoient et continuerent d'avaler juſqu'à ſe trouver bouche à bouche. La griſe me parut avoir l'avantage; peu après il lui fut diſputé, elle le recouvra, puis le laiſſa échaper, enſuite le reprit. Ces alternatives ayant duré pendant trois heures, je

and there was a time, during which the yellow Anemone was near being worſted; till at laſt, the grey one loſing hold of her end of the ſlice, the other carried off the prize. Yet, as ſhe ſucked in but ſlowly, the grey one ventured with her mouth upon a laſt tug at her end, ſtill in ſight, which ſhe had ſlipped; but this freſh effort proved fruitleſs: the yellow champion gave a laſt pull, and ſwallowed down the whole. During this whole ſtrife, the two parties did not ſeem to be animated by any other paſſion than that of ſnatching the ſlice of fiſh from one another; and though the two animals continued afterwards to remain neighbours, they lived very quiet and peaceable together.

Theſe animals are ſometimes very voracious. Could it be believed that the ſame creature, that can continue in pretty good plight for a whole twelvemonth, and perhaps longer, without taking in any other food, beſides what may

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vis un moment où les affaires de la jaune paroiſſoient aller aſſez mal, lorſqu'enfin la victoire ſe déclara pour elle: la griſe lacha le bout. Comme la jaune n'avaloit pas très vite, la griſe eut le temps de revenir à la charge; elle ſaiſit de nouveau par la bouche le bout, qu'elle venoit d'abandonner, mais ce fut inutilement; la jaune fit un nouvel effort, et le morceau fut avalé. Il ne parut dans ce combat ſingulier d'autres efforts que ceux qui tendoient à s'entre arracher la proye, et depuis les deux anemones toujours voiſines ont vécu auſſi paiſiblement qu'auparavant.

Cependant les anemones ſont quelquefois très avides. Croiroit-on que cet animal, qui peut vivre en aſſez bon état pendant une année, et peut-être plus, ſans prendre d'autre nourriture que celle qu'il trouve diſſéminée dans l'eau

may be diſſeminated in the ſea water, and does not ſeem very active to lay hold of it's prey, but rather waits patiently till chance throws it within reach of its limbs; that this animal ſhould ſtill be ſo greedy as to gulp down two whole muſcles, I gave one of them by piece-meal, and burſt the next day with indigeſtion, when it has a power of throwing up ſo eaſily what it has ſwallowed down? This was the caſe with an Anemone of the third ſpecies, and of a middle ſize, which had been fiſhed lately.

On the 8th of October, the ſame Sea-Anemonies (of which I had clipped out the upper part with the mouth and limbs, inſtead of which new ones were re-produced, perfect enough to enable them to eat) were divided a ſecond time, and again renewed, ſo as not to be diſtinguiſhed from thoſe which had undergone no operation. More than

l'eau de la mer, et qui ne paroit pas ſe donner de grands mouvemens pour attraper la proye qui l'environne, qui au contraire attend patiemment que le hazard la faſſe tomber entre ſes membres, ſoit aſſez glouton pour avaler en deux heures la valeur de deux groſſes moules, qu'on lui préſente par morceaux et crever d'indigeſtion le lendemain; tandis qu'il peut rendre aiſément ce qu'il avale? C'eſt ce qui eſt arrivé à une anemone de la troiſieme eſpece et de moyenne groſſeur, qui étoit récemment pêchée.

Le huit Octobre les mêmes anemones de mer, dont j'avois retranché la moitié du corps où ſe trouvent les membres et la bouche, et auxquelles toutes ces parties s'étoient reproduites, juſqu'à leur permettre de manger, ont été coupées de nouveau et ont repouſſé pour la ſeconde fois ſi bien, qu'il n'eſt plus poſſible de les diſtinguer de celles qui n'ont ſouffert aucune alteration

than that, having taken particular care to feed one of these halves clipped for the second time, I saw it grow stronger and stronger every day, and perform with equal facility the same functions as any other compleat original Anemone. The only difference or exception is, that its basis is not yet perfect enough, as to enable the animal to adhere or fix itself to the glass; I make no doubt however, but what this new Anemone will, in a short time, acquire the only powers yet wanting, to render it a perfect one; see fig. 16 and 17. Might not one ask, upon all these facts, what is become of the original animal? is it that which continued adhering by its basis to the glass? or is it the upper half? Are there animals among which an *individual* is not a *simple* being?

There oozes habitually out of these insects an excrementitious humour, which thickens round their body. At periodical times it issues out in a greater quantity, and assumes then a *vermi-*

teration. Il y a plus, ayant pris soin de nourrir une des parties retranchées, par la seconde coupe, je l'ai vue se fortifier de jour en jour; et faire avec la même aisance toutes les fonctions de l'anemone parfaite, excepté que la base n'est point encore en état de s'attacher, mais elle s'élargit et je ne doute point par les progrès que je lui vois faire que la nouvelle anemone n'aquiere dans peu le seul point qui lui manque, pour arriver à l'état parfait, voyez les figures 16 et 17. Ne pourroit-on pas demander maintenant qu'est devenu le premier animal? est-ce celui qui a été retranché, est-ce l'autre? y auroit-il donc des animaux où un invidu ne seroit point un être simple?

Il sort habituellement du corps de ces animaux une humeur excrémentitielle qui s'épaissit; mais elle en sort aussi périodiquement en plus grande abondance, et y prend alors une forme

cular appearance. During that period, the Anemone ſeems to be ſickly; but afterwards recovers its ſtrength.

It is upon this ſpecies that I had particular opportunities of obſerving, that when, by cutting, there remains but the baſis, and a ſhort ſtump of the body; the animal is equally, if not more, affected by the light, than before the operation. The ſame effect is produced upon the part where the mouth and limbs are.

I may perhaps be taxed with cruelty, with regard to theſe inſects. But, I believe, I ſhall ſoon be abſolved of the charge, when it is conſidered what favourable effects my experiments have had upon the Anemonies which have been the ſubject of them; as I have not only extended their exiſtence, but likewiſe renewed their youth; and this ſure is no ſmall advantage.

What

forme vermiculée; pendant ce temps l'anemone paroit malade, mais après elle reprend une nouvelle vigueur.

C'eſt particulierement ſur cette eſpece que j'ai eu lieu d'apercevoir que lors même qu'il ne reſte à ces animaux que la baſe et un tronc aſſez court, ils ſont autant et plus affectés par la lumiere, qu'avant d'être mutilés. Il en eſt de même de la partie, où ſont les membres et la bouche.

On pourroit être tenté de m'acculer de cruauté envers ces animaux, mais ſi l'on fait attention aux effets qu'ont produit mes expériences, ou ſoupçonnera aiſément que pluſieurs ne peuvent que ſe féliciter d'en avoir été l'objet; car non ſeulement j'ai étendu leur être, mais probablement je les ai rajeunis, ce qui n'eſt pas un petit avantage.

 Ce

What I have mentioned of the effects of fresh water upon the first species of Sea-Anemonies, of the colour of the *re-produced* limbs, of their way of swallowing whole live muscles, &c. must be likewise understood of, and applied to this third species; and, in many respects, to the second.

I am sensible that many things are still wanting in this paper; but what I have wrote down, is very near all that I can infer from my first observations and experiments, without exposing myself to the necessity of falling back in regard to essential things.

POST-

Ce que j'ai dit des effets de l'eau douce sur la premiere espece, de la couleur des membres reproduits, de la maniere d'avaler les moules, &c. doit s'appliquer à celle-ci, et en grande partie à la seconde.

Je laisse sans doute beaucoup à desirer ici, mais ce qui y est exposé est presque tout ce que je puis conclure de mes premieres observations et de mes premieres expériences, sans risquer de me mettre dans la necessité de revenir sur mes pas pour des choses essentielles.

POST

POSTSCRIPT.

Havre de Grace, April 28, 1773.

JUST as I concluded this eſſay, I found out a *fourth* ſpecies of Sea-Anemonies, of the ſize of the ſecond, and of an elegant form, having the appearance of a cluſter of white or fleſh-coloured feathers. Theſe Anemonies are found in oyſter-beds, &c. I have obſerved that there grows or comes out of their body and mouth a ſort of threads about the ſize of a horſe-hair, which being examined with a ſolar microſcope of five inches diameter, appear as if made up of a prodigious number of veſſels, wherein a liquor is ſeen to circulate. The largeſt

POST SCRIPTUM.

Le Havre, 8 Avril, 1773.

LORSQUE je terminois ce mémoire, j'ai découvert une quatrieme eſpece d'anemones de mer, groſſe comme la ſeconde, et d'une forme élégante, repréſentant comme un aſſemblage de panaches blancs ou couleur de chair. On la trouve dans les huitrieres, &c. J'ai remarqué qu'il ſort de ſon corps et de ſa bouche certains cordons de même couleur déliés comme un crin, leſquels vus au microſcope ſolaire ſous un diametre de 5 pouces, paroiſſent formés d'une quantité innombrable de vaiſſeaux où circule une liqueur. Les plus gros s'uniſſent

largeſt of theſe unite together, much in the ſame manner as the optic nerves do in man. Such an organization is doubtleſs intended for moſt important purpoſes. Some young ones of this ſpecies, which ſtill adhered to one another by a ſtring of communication, ſhut themſelves up in the ſame inſtant, when this ſtring was touched in the middle. As I could not directly contrive a total ſection of this large ſpecies, I tried it upon the young ones; and theſe ſhooted out again after the operation; (and ſo have the old ones done ſince). I have met with a ſort of monſter among theſe Anemonies, *viz.* one which ſeemed to incloſe or contain three others, two of which were united at their baſis, and the third lay, as it were, concealed in the folds.

Nature has reſources little known to us; it ſeems ſometimes to vary its operations, with an intent, as it were, the more to ſtimulate our curioſity

s'uniſſent en ſe rencontrant à peu près comme les nerfs obliques dans l'homme; une telle organiſation eſt ſans doute deſtinée à des uſages précieux. Dès petits de cette eſpece, qui étoient encore adhérens l'un à l'autre par un cordon de communication, ſe fermoient en même tems lorſque je touchois ce cordon par le milieu. Ne pouvant d'abord ſoumettre cette grande eſpece à une ſection totale, je l'ai eſſayée ſur de jeunes individus, ils ont repouſſé; (les grands repouſſent de même). J'ai eu dans cette eſpece un monſtre, c'eſt-à-dire une anémone qui paroiſſoit en contenir trois, dont deux oppoſées par leur baſe et une troiſième cachée dans les plis.

La Nature a des reſſources qui nous ſont peu connues, et ſemble quelquefois varier ſes procédés à deſſein de piquer de plus en plus notre curioſité, et peut-être de dévoiler ſes ſecrets,

ceux

riosity, and perhaps to disclose her secrets to those who are endowed with a degree of sagacity or patience, sufficient to follow and investigate the effects offered to our observations. Amidst the great number of Anemonies of the third species which I have clipped across the body, there have happened to be two, whose lower part has in the usual way shot forth new limbs: but the *upper half*, where the limbs and mouth were, instead of healing up into a new *basis*, has produced both another mouth and limbs. Hence an animal was formed, which caught its prey, and fed at both ends in the same time.

The Sea-Anemonies of the three first species mentioned before, and perhaps those likewise of the fourth, feed upon those floating transparent animals of a white glassy, or of a blue or purplish hue, called *wandering nettles*, or *sea-jellies*. An Anemone of a middle size, of the first and third species,

ceux qui ont assez de sagacité, ou même de patience pour suivre les effets qu'elle nous présente. Dans le nombre considerable d'anemones de la troisieme espece que j'ai coupées par la moitie du corps, il s'en est trouvé deux, dont la moitie inférieure a repoussé de nouveaux membres comme à l'ordinaire: mais la moitié supérieure où étoient les membres et la bouche, au lieu de se consolider et de former une base à la section, y a reproduit des membres et une bouche, de sorte qu'il en a resulté des animaux, qui saisissent la proye, et mangent par les deux bouts en même temps.

Les anemones de mer des trois premieres especes, et peut-être ceux de la quatrieme, se nourrissent de ces animaux flotans et transparens, le plus souvent de couleur de verre blanc, ou bleux, ou violets, connus sous les noms d'*Orties errantes*, *de gelées de mer*, &c. Une anemone de la premiere et de la troisieme espece comme

ſpecies, ſuch as that repreſented by Figures 1, and 13, ſwallows one of theſe animals of the ſize of half an orange. All theſe four ſpecies are good to eat.

Particular EXPLANATION of ſome of the FIGURES.

Plate XVI. Fig. 14. ſhews the Anemone of the third ſpecies, when ſhrunk up. One ſees round it a ring of ſand and broken pieces of ſhells ſticking together by means of the excrementitious humour habitually oozing out of the body of the animal, or out of the little granulated knobs, with which it is covered towards the upper part. This ring is alſo to be ſeen in the ſame Anemone, when lengthened out, as expreſſed in Fig. 13.

Fig.

comme celles des figures 1 et 13. avalent un de ces animaux gros comme la moitié d'une moyenne orange. Toutes ces 4 eſpeces d'anemones de mer ſont très bonnes à manger.

EXPLICATION de quelques unes des FIGURES.

Planche XVI. On voit à la figure 14 une anemone de la troiſieme eſpece, dans ſon état de contraction. L'eſpece de cordon, qui paroit tout autour, eſt formé de ſable et de fragmens de coquillages joints enſemble par cette humeur excrémentitielle, qui ſort habituellement du corps de l'animal, ou des petits mammelons dont la partie ſupérieure eſt revêtue. Cet anneau s'apperçoit auſſi dans la même anemone, lorſqu'elle s'allonge, fig. 13.

Dans

Fig. 10. ſhews a Sea-Anemone, of the ſecond ſpecies, concealed under the ſand, and covered over in different places with broken ſhells and gravel, with which the animal forms a coat of mail to ſecure itſelf under, but out of which it can ſlip in an inſtant. The figure ſhews it when it ſpurts out water at its mouth, and at the end of its limbs.

Plate XVII. Fig. 11. ſhews the ſame Anemone open. The mouth is in the center of the upper part: it is not always ſhaped in the ſame manner in other Anemonies as it is ſeen here, or at leaſt does not always appear to be ſo. Figure A ſhews a mouth as engraved from another Anemone, but which alters or ſhifts its form every moment. This Anemone has got five rows of limbs. There are ten of them in the innermoſt row; the like number in the ſecond; twenty in the third; thirty in the fourth; and fourſcore in the fifth.

When

Dans la figure 10 de la même planche ſe voit une anemone de mer, de la ſeconde eſpece, cachée dans le ſable, et couverte en divers endroits de coquillages briſés, et de gravier, dont cet animal ſe fait une eſpece de côte d'armes, ſous laquelle elle ſe cache, mais dont elle peut ſortir en un inſtant. La figure la repréſente dardant de l'eau de ſa bouche, et de l'extrémité d'un de ſes membres.

La figure 11 de la XVII. planche montre la même anemone ouverte. La bouche eſt au centre de la partie ſupérieure: cette bouche n'eſt pas toujours formée de la même maniere dans d'autres anemones, ou du moins ne le paroit pas toujours. Vous voyez en A une bouche deſſinée d'après une autre anemone, et qui à chaque inſtant change de forme. Cette anemone a cinq rangs de membres: le rang intérieur et le ſuivant en ont chacun dix; il y en a vingt dans le troiſieme, trente au quatrieme, et 80 au cinquieme.

Quand

When the animal is out of the water, and is ſqueezed, it ſpurts out water at the mouth and at ſeveral of its limbs at the ſame time; ſo that it imitates pretty well the play of water-works. When the limbs are drawn in cloſer together, they give it the look of a flower, eſpecially of an Anemone.

Fig. 12. ſhews an Anemone of the ſame ſpecies, turned inſide out, as when a purſe or ſtocking is ſo. A thin tranſparent membrane, with white ſtripes, lines the whole inſide of the animal; and through it are ſeen the bowels, part of which hang or come out at the middle. One may obſerve, beſides, in this figure, two hollows ſinking in, which are formed by two pretty ſtrong cartilages.

Quand l'animal eſt hors de l'eau, et qu'on le preſſe, il jette de l'eau par la bouche, et pluſieurs de ſes membres en même tems, imitant aſſez bien ainſi le jeu des jets d'eau. Lorſque les membres ſont ſerrés l'un ſur l'autre, ils donnent à l'animal la figure d'une fleur, et ſurtout d'une anemone.

A la figure 12, de la même Planche, vous voyez une anemone de la même eſpece, mais tournée du dedans en déhors, comme un bas ou une bourſe. Tout le dedans eſt revêtu d'une membrane fine et tranſparente, avec des rayes blanches, les inteſtins paroiſſent au travers, et une partie ſort du milieu et pend au déhors. On voit auſſi deux ouvertures, qui penetrent dans l'intérieur, et ſont compoſées de deux cartilages aſſez forts.

Fig 11

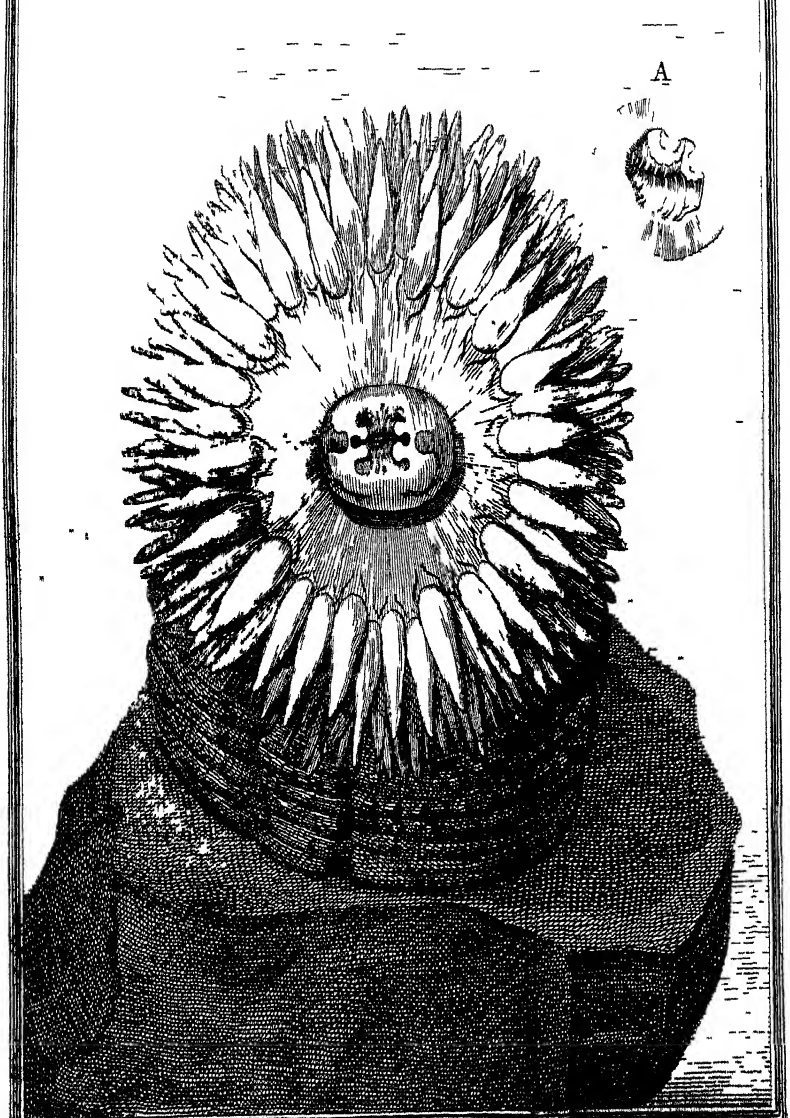
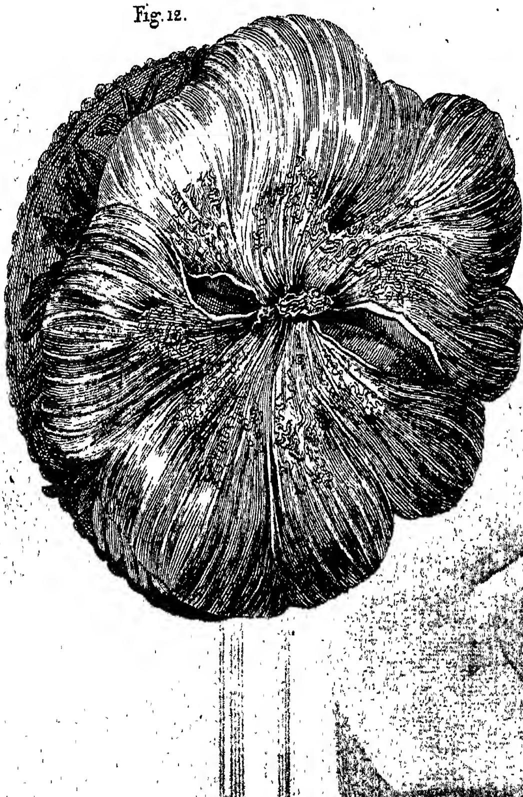
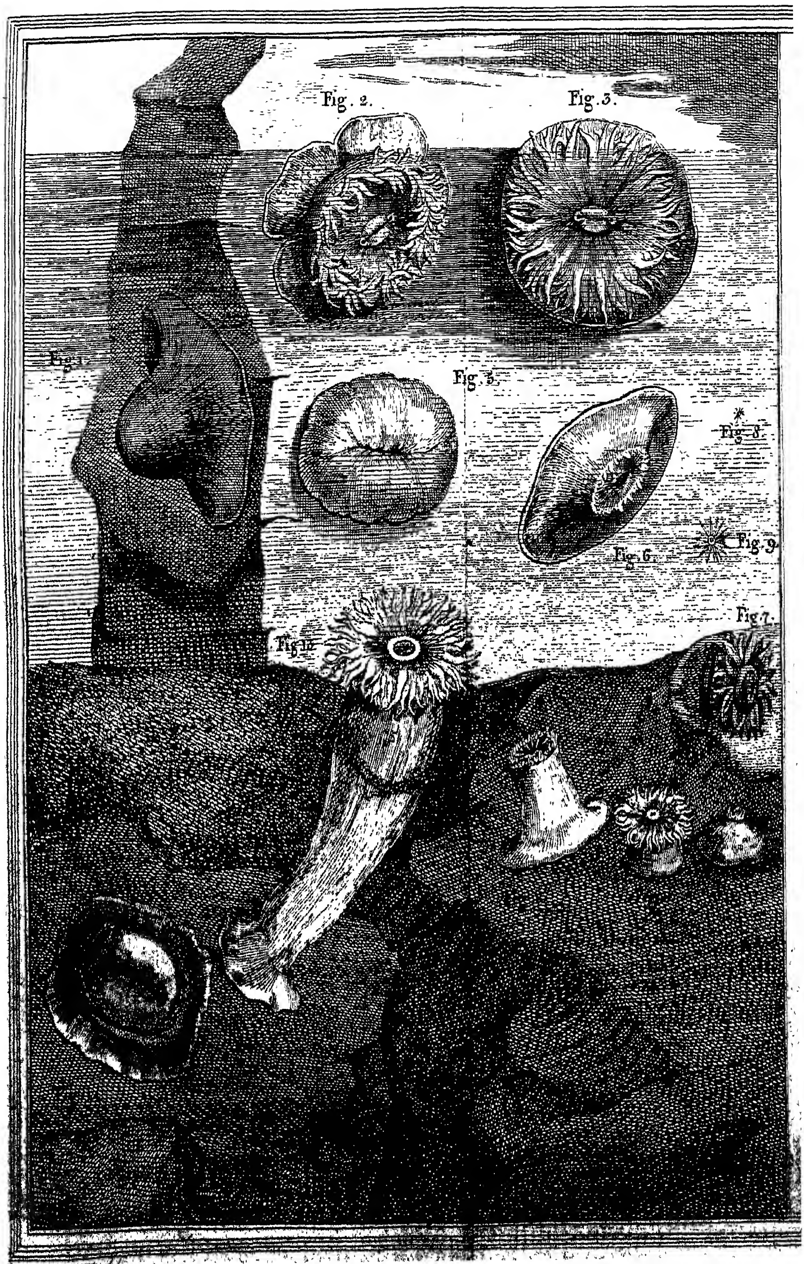


Fig. 12.



J. Basire sculp.



DICQUEMARE

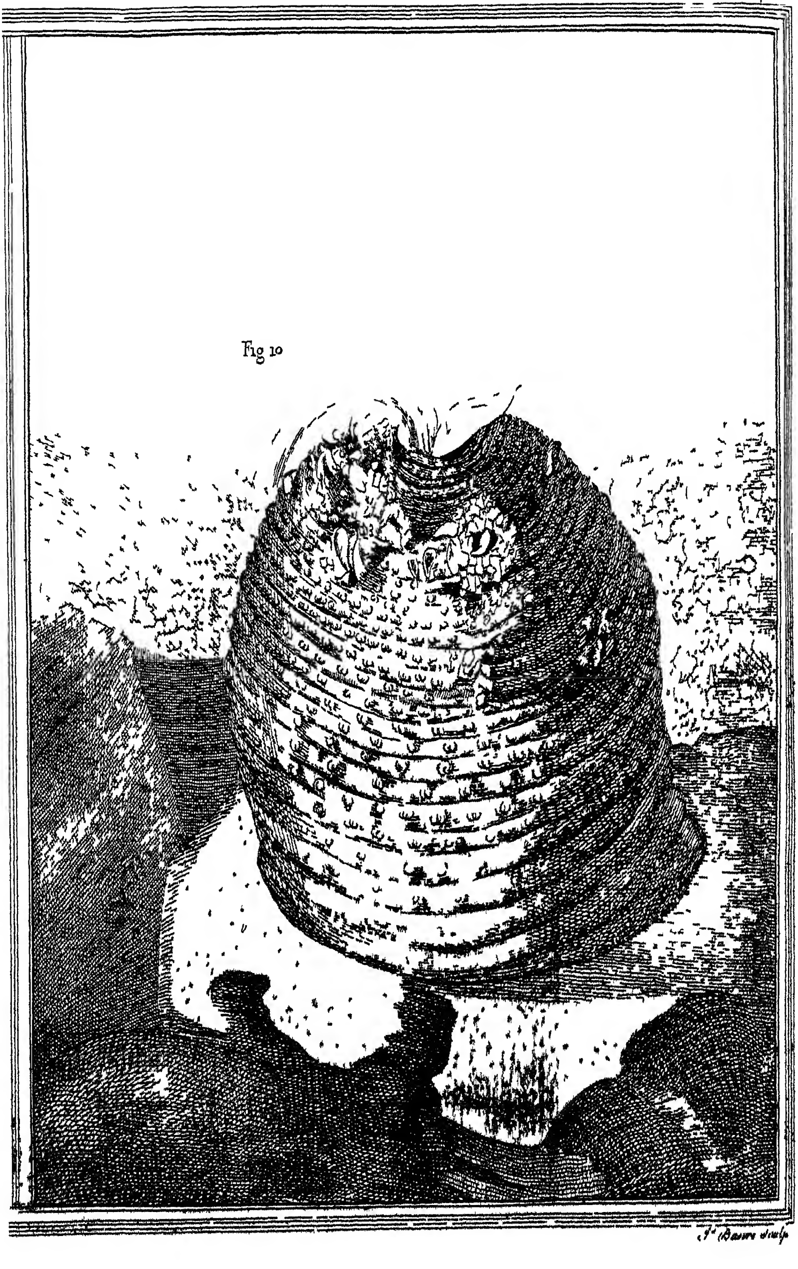


Fig. 10

Jas. Basire sculp.

N. B. Dr. Solander, F. R. S. being consulted about these sea-worms, which are evidently of the class of the Actinia, referred the first species, Tab. XVI. Fig. 1—3. to the *Actinia Equina* Linn. Syst. Nat. 1088. 1. the second species, Tab. XVI. Fig. 10. and T. XVII. Fig. 11 and 12. to the *Actinia Senilis*. Ib. 1088. 2. and the third, Tab. XVI. Fig. 13 and 14. to the *Actinia felina*. Ib. 1088. 3.

XXXVIII. *Account of a new Hygrometer. By* M. J. A. De Luc, *Citizen of* Geneva, *F. R. S. and Correspond. Member of the Academies of* Paris *and of* Montpellier.

Read June 10, 1773.

IN laying before the Royal Society an account of my attempts to find out a method for meaſuring the moiſture of the air, I think myſelf obliged to relate the gradual ſteps of my mind, the obſtacles I met with, the means by which I endeavoured to overcome them, the degree at which I flatter myſelf to have arrived, the hopes that may be entertained of farther advances, and the uſes which may be derived from my firſt experiments.

Attempts to invent an HYGROMETER.

1. In order to proceed regularly in this inveſtigation, I began by examining the eſſential requiſites in a machine intended to meaſure humidity, which I found to be the three following:

1ſt, The ſettling of a fixed point, from which every meaſure of the ſame kind ſhould be taken, ſuch, for inſtance, as that of boiling water in a thermometer, when the barometer is at a certain height.

2d, Degrees equally determined, or comparable, in different hygrometers, ſuch as are in the thermometer,

meter, the ſcales of Fahrenheit, Deliſle, Reaumur, &c.

3d, Conſtancy in the variations produced by the ſame differences of humidity.

2. I perceived, moreover, that it were to be wiſhed, that the hygrometer ſhould give a true indication of the relation between the real quantities of the humidity, or at leaſt between their differences: but this laſt point I rather conſidered as a deſirable degree of perfection, than as an indiſpenſable requiſite; the eſſential point being, that obſervers might underſtand each other, when mentioning degrees of humidity; and this ſeemed to be ſufficiently provided for by the abovementioned conditions.

3. Having thus planned to myſelf the work I had to go through, I firſt attended entirely to the firſt point, and laid aſide all the others. This I again ſubdivided. I had ſoon perceived that I muſt begin by thinking much leſs of the hygrometer than of the different phænomena of humidity. For this purpoſe it was neceſſary to find out a fixed ſtate, either of bodies in general, or of ſome body in particular; and this fixed ſtate might either be extreme humidity, or dryneſs, or any intermediate point.

4. Knowing that the extremes in nature are commonly very difficult, and ſometimes impoſſible to hit, I conceived at firſt greater hopes of intermediate degrees. But in vain did my imagination fatigue itſelf in a road, which I was forced to abandon.

5. I then came to the extremes, and that of abſolute dryneſs was the firſt I was induced to try. But having found no other way to procure it but by fire, and fire not producing it in all bodies which appeared to

to me ſuſceptible of humidity, but by altering their nature, I reluctantly perceived that I ſhould be obliged to look for my firſt point, where I had the leaſt hopes of diſcovering it.

6. I remained a long time without diſcovering any thing in this new road; and very often turned back, but was always obliged to return to extreme humidity, as to the only part of my object, of which I could poſſibly get any hold.

7. The words, which are neceſſary for communicating our ideas to others, are often obſtacles to the raiſing of new ideas in ourſelves. They are by far too few to expreſs diſtinctly every ſhade of intellectual objects. Humidity was a word which I conſtantly repeated to myſelf, and it conſtantly led me to a claſs of phænomena, in which I could find nothing ſettled.

8. Water at length preſented itſelf to my mind; and in this fluid, which to all appearance ought firſt to have ſtruck me, I beheld with ſurprize, what I had been labouring, through many a round, to diſcover, under the denomination of extreme humidity. I was not at that time conſidering humidity in any particular phænomenon; I only obſerved that it was conſtantly produced by aqueous particles diſſeminated through bodies; and I found in water the maximum of the approach, and conſequently of the action, of theſe particles.

9. In order now to avoid the ambiguities from whence, in my opinion, the difficulties in theſe matters ariſe, let me be allowed for the future to employ no words but ſuch whoſe meaning is well determined. Humidity will accordingly be no more

that

than an effect, or modification of bodies from a substance more or less abundant, but constantly consisting of aqueous particles under different forms. This substance, considered in its utmost extent and under all the appearances which it assumes in nature, I shall express by the Latin word *humor*. Thus ice, water in its different degrees of heat, hail, snow, icicles, rain, dew, clouds, fog, mist, invisible vapours, are no more than modifications of this same substance, different species of a determined genus; since aquosity, which is common to all, is its generic character.

10. The more humor there is in any body, the more humid that body is; and consequently if it be plunged in water, and soaked so as not to be able to receive any more, it is got to extreme humidity, the water which fills up all its pores being humor in the highest degree of intensity.

11. Not, however, but that discrete humor, or vapour of every kind, may in some respects produce as great effects as concrete humor or water: but there is always some difference in some other respect, and chiefly in regard of time. Bodies encompassed with air are continually discharging, by evaporation, part of the humor they imbibe from it. If the circumstances are such, that the humectation exceeds the evaporation, the body at length wets through*, more or less quickly as the quantity of humor which it receives in a given time is greater or less, and likewise in proportion as this quantity exceeds that

* By *wetting* here, I understand arriving at the greatest degree of humidity.

which

which evaporates. It is ſuddenly wetted, when the humor is ſo condenſed as to become water, becauſe the evaporation which takes place at the ſurface of the water, does not weaken its action on the bodies dipped into it; it is only wetted little by little, or, what happens ofteneſt, in part, when the humor is diſcrete or reduced into vapour; becauſe, while it is depoſited on particular ſpots, it evaporates from the interſtitial parts, and that more or leſs according to the ſtate of the air, and that of the moiſtened bodies.

12. This difference, however, in point of time, between the action of concrete and diſcrete humor, only takes place on the ſurface of bodies, or at a ſmall depth; it diminiſhes, and may even become oppoſite, as the depth of bodies increaſes, becauſe the diſcrete humor is then more eaſily introduced into their pores than water, which more than makes up for their different intenſity.

13. This conſideration ſolves a difficulty, which at firſt puzzled me. I had been told by bird-catchers, that the threads of thoſe nets which they caſt on the water-ſide, were leſs ſtretched from the action of water than of dew. Hence it might ſeem, that what I took for the extreme of humor had leſs effect than what is only a degree of it. But two particular cauſes accounted for this difference.

1ſt, The air contained within the fibres of the thread oppoſes the introduction of the water, which, preſenting itſelf in a body, ſhuts up the paſſages by which the air ſhould eſcape to give it room; but it yields to the drops of dew, which permit its eſcape while they penetrate through the threads.

2d, Another particular caufe, lefs obvious though not lefs probable than the preceding, is the difference in the mutual attraction of parts, in the concrete and difcrete humor, and confequently in their refpective facility to feparate, and get one by one through the narrow pores. When this entrance is attempted by the humor, under the form of water, the mutual attraction of its parts, being greater than in dew, occafions a greater refiftance to their introduction, than when they are already divided by fome other caufe, viz. when the humor is reduced to fmall drops, or vapor.

14. This phænomenon, therefore, does not contradict my principle; it is only a particular fact; and it remains true, that bodies furrounded with water are expofed to the extreme of humor. To remove this caufe of exception from my hygrometer, it was fufficient to provide outlets for the air, and not to increafe too much the thicknefs of the body, upon which the humor was to act.

15. Another difficulty, which prefented itfelf, was that water might probably act with more or lefs energy in proportion to its heat. But this did not ftop me long. As my prefent object was a fixt point for the hygrometer, and not the greateft power of water, confidered as a caufe of humidity, it was enough to employ it conftantly at the fame degree of heat; and, to fix this with greater precifion, I determined to ufe water at the inftant that it ceafes to be ice. The bafis therefore of my hygrometrical fcale was to be the foaking power of melting ice.

16. This principle, being thus unfolded, appeared fo fimple, that I was at firft furprifed how it could have

have been ſo long overlooked. But I afterwards accounted for this, from the difficulties which I met with in the diſcovery. The notion of an hygrometer being both complex and unſettled, all the obſtacles preſented themſelves at once, and this multitude of ideas exceeded the power of attention. The very firſt ſteps were apt to miſlead. On the one hand, I looked for an hygrometer with a head full of the matters already uſed for hygroſcopes, which always are more or leſs altered by water; and, on the other, the name of humidity was applied to that cauſe, the effects of which I wiſhed to meaſure; and both points of view turned aſide the mind from the idea of water, as being proper to afford the required fixed point in an hygrometer.

17. The firſt difficulty had not eſcaped me; but, conſidered in itſelf, it did not appear unſurmountable. I was in hopes that a ſubſtance might be found capable of being affected by the ſoaking power of water, without being altered by it. As the nature of this ſubſtance was to determine not only the form of the hygrometer, but alſo the ſpecies of the degrees, which were to indicate the different quantities of humor, I concluded that my ſecond object ought to be the diſcovery of this ſubſtance.

18. In this reſearch, I again divided the objects, by conſidering ſeparately the three kingdoms, viz. the mineral, the vegetable, and the animal. The two firſt offered no ſubſtance fit for my purpoſe, viz. none that would obey the impreſſions of humor, without being altered either by it or by other cauſes. But in the animal kingdom bones drew my attention; and ivory, in particular, ſeemed to poſſeſs the required

required qualities. I had obſerved that the key of an ivory cock was tighter or ſlacker, as there was more or leſs humor in the air. Ivory pallets, uſed for water-colours, ſhewed no alteration, at leaſt none that was laſting. I knew alſo the elaſticity of this ſubſtance, which ſeemed to ſecure its coming back to the ſame ſtate, on its return to the ſame degree of moiſtneſs.

19. There ſtill remained on this ſecond head another object of inquiry, which was almoſt neceſſarily connected with the third, viz. the ſpecies of the degrees to be given to the hygrometer. The beſt form to be given to the ivory, in order to receive with eaſe the impreſſions of the humor, and to have its effects meaſured upon it, was to be determined. I firſt thought of ivory rods, the lengthening of which ſhould be meaſured by a machine ſimilar to the pyrometer. I likewiſe had ſome notion of a large nonius, formed of an ivory and a metallic rod. Either of theſe machines would admit of a fixed graduation, as both the dimenſions of their parts and proportions to one another could be determined. But then I apprehended that ivory might perhaps, like wood, have its longitudinal fibres but little liable to be extended by the humor, and that the imperfections of theſe two kinds of micrometers would occaſion a conſiderable irregularity in the hygrometrical degrees. I alſo feared that if ivory rods were made thick enough to prevent their bending, ſuch a thickneſs might become an obſtacle to their intire penetrability by the humor (14). I therefore concluded that the ivory ſhould have ſuch a form, that, though very thin, it might not warp; and that the meaſurable varia-

tions were to be the removal or approach of its fibres to each other.

20. Being thus guided by these necessary conditions, I thought of different thin ivory cups, the capacities of which should be measured by quicksilver; and at last imagined a hollow cylinder, in which the variation of its capacities, when more or less moist, might be measured by the quicksilver it should be filled with; and which putting into a glass tube joined to the ivory one, would of course rise more or less, as that vessel was more or less deprived of humor.

21. Nothing now remained but to find out a way of estimating the changes of capacity of the ivory tube, by means of the variations in the height of the mercury in the glass tube. I thought, at first, that by using very nice scales, in order to compare the weights of the mercury contained in the cylindrical vessel, with that of a column of the same liquid in the tube, I might obtain the proportions of these weights with a sufficient exactness, to be able to measure the variations of the mercurial column, by degrees representing aliquote parts of the whole mass.

22. This in itself was undoubtedly an exact method; but then it required in the execution such a nicety in the scales, that I durst not employ it in the construction of an instrument of so extensive an use. Such scales are always scarce from their high price. I remembered to have myself found that inconvenience in the construction of a Delisle's thermometer, and concluded I must hit upon some method to

23. The idea of a thermometer, which struck my mind, was a lucky one. I was led to it by a kind

kind of connexion between the ſcale of that inſtrument and that of my hygrometer. I ſoon perceived, that, by applying to my hygrometer a thermometrical tube, already graduated by means of two fixed points of heat, it would only be neceſſary to know the proportion of the weights of mercury in this thermometer, and the hygrometer, to which its tube was to be applied, to have in this laſt inſtrument degrees as well determined as in the firſt. Scales of a common degree of exactneſs were ſufficient to eſtabliſh between the reſpective degrees of both inſtruments a proportion equal to that of their mercurial weights (42 and 43).

24. Beſides the eaſe in the execution, this contrivance afforded me a very ſimple method to correct the effects of heat upon the mercury contained in the hygrometer. It is indeed obvious, that, abſtractedly of the effects of the humor, the new inſtrument muſt in itſelf be a regular thermometer; and that conſequently the variations of an adjoint thermometer were immediately to point out this correction.

25. Every principle being thus ſettled, nothing remained but to contrive its conſtruction. I began by making ſome experiments, on the nature and quantity of the action of water upon ivory. I made for that purpoſe a ſmall cylindrical ivory veſſel, of an inch in diameter, and eight lines in length, and reduced its thickneſs to leſs than ¼ of a line. I likewiſe prepared a wooden cylinder, equal in its diameter to the internal one of the veſſel: I then put this veſſel into water, in ſuch a manner that it only wetted it outwardly to the rim. In a very ſhort time the wooden cylinder, which at firſt filled the

cup exactly, no longer filled it. After a few hours, I perceived that the internal ſurface grew wet, and by means of a magnifying glaſs, found it covered with a very fine dew. This dew did not encreaſe by the veſſel remaining any longer in the water; the evaporation being doubtleſs equal to the tranſudation; and the capacity of the veſſel, which encreaſed till the appearance of the dew, ſeemed afterwards at a ſtand.

26. This tranſudation puzzled me a little; it ſhewed me that the water would get into my hygrometer, which at firſt appeared an inconvenience. I ſoon, however, found an advantage in it. The water, after having ſoaked through the ivory, would immediately puſh back the mercury, which having by degrees ſunk in the tube, during the penetration of the water through the pores of the ivory, muſt thus riſe again. Hence I might expect a maximum for the fall of the mercury, very eaſy to be determined. As for the water introduced into the ivory veſſel, I was in hopes that it would go back as ſoon as the outſide of the cup ſhould be dry.

27. Having thus aſcertained that ivory was very eaſily affected by the impreſſions of the humor, it ſtill was neceſſary to know, whether the variations of the one would always equally anſwer to thoſe of the other. Having accordingly taken my ſmall cup out of the water, and expoſed it to the air, I ſoon found that its capacity diminiſhed, but that even after ſeveral days it did not return to its former ſtate. This again puzzled me; but I ſuſpected that the external preſſure of the tool upon the ivory might ſomewhat have compreſſed it, and that the water having reſtored the ivory fibres to their original pitch, the abſolute

absolute capacity of the cup remained larger than it was before.

28. To satisfy myself about this, I got another wooden cylinder, which filled the capacity of the vessel in its present state. This I again put into the water, and left it there a sufficient time; I then exposed it to the air to be dried; and after that found that the wooden cylinder filled it as before. Hence I concluded, that in the construction of my hygrometer the ivory cup should be dipped for some time in water, and afterwards dried, before it was used.

29. Thus having cleared up my conjectures, as much as they could be, by these preliminary experiments, and got some insight into the proportions of the different parts of the machine, I proceeded to its construction, and finished it in the following manner:

Description of an HYGROMETER.

30. Tab. XVIII. figure 1. shews the section of the interior part of the instrument, of its true length, in the direction of its axis.

The first part to be described, being in some measure the soul of the hygrometer, is an ivory tube, *a a b*, open at the end *a a*, and closed at *b*. It is made of a piece of ivory, taken at the distance of some inches from the top of a pretty large elephant's tooth, and likewise at the same distance from its surface, and from the canal which reaches to that point (68). This piece is to be bored exactly in the direction of its fibres; this hole is to be very streight, and its dimensions are $2\frac{1}{2}$ lines in diameter, and two inches 8 lines in depth from *a a* to *c*.

31. Prepare

31. Prepare after this a brass cylinder, about $3\frac{1}{4}$ inches long, and to one of its extremities fix the pully proper to receive the string of the bow when the piece is turning. This cannot be done too carefully, both to make it perfectly round, and to fit it exactly to the hole of the ivory tube; its extremity must even be rounded, that it may be applied closely to the bottom of the hole. Having then roughly prepared the outside of the ivory tube, and introduced into it the brass cylinder, put both pieces thus united upon the turning wheel, and find out on the outside bottom of the ivory tube, the point which answers to the axis of the brass piece, in order that this may turn exactly upon its axis. It is with this view that the brass cylinder is made longer than the ivory tube.

32. All these precautions are designed to make the sides of this tube of an equal thickness, viz. $\frac{3}{16}$ of a line, except at the two extremities. At the bottom *b* the tube ends in a point, and at the top *a a*, it must for about two lines be left a little thicker, in order to enable it to bear the pressure of another piece, which is to be put into it. Thus the thin or hygrometrical part of the tube will be reduced to $2\frac{1}{2}$ French inches, including the concavity of the bottom.

33. Before this piece is used, put it into water so as that the external part alone be wetted by it, and leave it there till the inside be every where covered with the dew I mentioned before (25). This will take place in a few hours; I have given the reasons for this operation (28).

34. The glass tube intended for this hygrometer must be about 14 inches long. Its lower end is seen in

in *d d e e* (fig. 1.). Its internal diameter is about 3/[illegible] of a line. The reaſon why it ſhould not be ſenſibly leſs will be given hereafter (52); and if it was ſenſibly larger, the variations of height in the mercurial column would not be conſiderable enough. On the dimenſions that I propoſe, when the hygrometer is put into melting ice, in a fine ſummer day, the mercury falls about ſix inches in the tube. The outſide diameter of this tube ſhould be about two lines, in order that the part *g g* of a braſs piece through which it paſſes, and which is to enter into the ivory pipe, be as thin as poſſible.

35. The glaſs tube, as I ſaid before, ſhould have belonged to a thermometer. Its extremity widens of courſe towards the ball; which will be of uſe, when the mercury is poured into the hygrometer, in order that it may drive the air before it, by riſing from the ivory pipe into the glaſs tube. To preſerve this widening, break the ball of the thermometer by ſtriking againſt the bottom; and with pinchers take off the reſt little by little, and make the extremity cylindrical by grinding it upon the wheel. The ſame muſt be done at the top, which I ſuppoſe to have been made to end in an olive or ſmall reſervoir for the filling of the thermometer. This widening is likewiſe to be ſaved for the reaſons hereaſter to be mentioned (52).

36. The piece *f f g g* is intended to join the ivory with the glaſs tube. It is of braſs, ſhaped as in the figure. A cylindrical hole is bored through it, which holds the glaſs tube as tight as poſſible, without danger of breaking it; and its lower part is to enter with ſome degree of difficulty into the ivory pipe.

37. To

37. To hinder the part of that tube, which encloſes the braſs piece, from being affected by the variations of the humor, which might ſometimes prevent a ſufficient preſſure, I cover this part of the tube with a braſs verrel, repreſented in *h h i i*. It muſt enter with force, and will henceforth be conſidered as part of the ivory pipe.

38. To unite thoſe pieces together, I make uſe of gum lac, or of maſtich, which melts by the heating of the glaſs and the braſs. I firſt cement the braſs piece with the glaſs tube by introducing the tube, and leaving it at firſt at an inch diſtance from the place where it is to be fixed; I then hold this end of the tube over live coals, by bringing it nearer and nearer, and turning it, that both that and the braſs piece be every where equally heated; and when they are hot enough to melt the gum lac, I rub the glaſs tube with it, and puſh the braſs piece to its place by means of a hollow bit of wood, drawn beforehand over the tube for this purpoſe. As the braſs piece advances, the lac accumulates towards the end of the tube; I take away the ſuperfluous part, but leave a ſlight coat of it over the end of the braſs piece, in order to preſerve it from the contact of the mercury that might corrode it. When this piece is properly placed, and ſtill warm, I cover with lac its cylindrical outſide, and introduce it into the ivory tube, which has been ſomewhat warmed by holding it near the fire, in order that the lac may ſtick more cloſely to it. As ſoon as theſe pieces are cold, they are found very ſtrongly cemented together, and neither mercury nor water can make their way between them.

39. The introduction of the mercury is the next operation. I first roll a slip of paper three inches wide over the glass tube, and tie it fast to the extremity which is nearest to the ivory pipe. I then introduce into the tube a horse hair long enough to enter the cylinder by one end, and to have the other rise three or four inches beyond the orifice of the tube. I then raise the paper which has been shaped round the tube, and use it as a funnel to pour the mercury into the instrument, which I hold upright. The purest quicksilver ought to be employed for that purpose, and it will therefore be proper that it should be revivified from cinnabar. I poured it then into the paper funnel, from whence it easily runs into the tube, with the assistance of some gentle shakes. The air which it drives before it comes out along the horse hair. Fresh mercury must from time to time be supplied, to prevent the entire emptying of the paper tube, and the running in of the mercurial pellicle, which the contact of air always produces upon the surface.

40. Some air bubbles generally remain in the tube; they may be seen through the ivory pipe, which is thin enough to have some transparency. These being collected together by shaking, must be brought to the top of the tube, and expelled, by means of the horse hair. To facilitate this operation, some part of the mercury must be taken out of the tube, in order that the air may be less obstructed in getting out, and the horse hair have a freer motion to assist it.

41. Air, however, cannot be entirely driven out in this manner. It is the weight of the mercury, with

with which the tube is for that reaſon to be filled, that in time completes its expulſion, by making it paſs through the pores of the ivory. To haſten this, I place my hygrometers in a box made on purpoſe; and this I fix pretty nearly in a vertical direction, to the ſaddle of a horſe, which is ſet a trotting for a few hours. The ſhakes ſometimes divide the column of mercury in the glaſs tube, but it is eaſily reunited with the horſe hair. When, upon ſhaking the hygrometer vertically, no ſmall tremulous motion is any longer perceived in the upper part of the column, one may be ſure that all the air is gone out.

42. I now come to the operations requiſite to make the ſcale of the hygrometer, and firſt of all to that which determines the baſe (15). This may be done as ſoon as the air is gone out. I then ſuſpend the inſtrument in a veſſel filled with ice mixed with the water it produces in melting. I take care to ſupply the melting by recruits of freſh ice, during the courſe of this proceſs, which laſts ten or twelve hours. In the firſt hour, the mercury ſinks above one third of the ſpace it has to go through; it advances leſs in the ſecond; and its motion leſſens thus gradually, till it appears ſtationary, which frequently happens after ſeven or eight hours, and it remains two or three hours in that ſituation. The ivory being then become more tranſparent on account of humidity, a very thin dew is perceived by a certain play of the light on the ſurface of the quickſilver. Laſtly, the mercury begins to reaſcend; the operation is terminated; and ſmall drops of water, as I expected, are at that inſtant ſeen upon its ſurface (26).

43. I follow the laſt ſteps of the mercury in its fall, by means of a fine ſilken thread fixed very tight around the tube. This is left at the loweſt point it has been brought to. If this point be too low, relatively to the frame of the hygrometer, freſh mercury is poured in, and the thread proportionally drawn up higher; if too high, I take off ſome of the mercury and lower the thread; and in both caſes make uſe of the horſe hair. This muſt be done when the mercury ceaſes to fall, in order that the place where the thread is to remain may be immediately determined by this operation.

44. This point thus fixed is named o in my hygrometer; it is that in which dryneſs is nothing (if I may be allowed to expreſs myſelf ſo), ſince it is that of extreme humidity, in a given heat; viz. that of melting ice. From this point are reckoned all the degrees I am now going to ſpeak of; which thus become degrees of exſiccation.

45. The laſt eſſential operation is that by which the ſize of the hygrometrical degrees are determined; and this I ſhall deſcribe by an example. It muſt be remembered that the hygrometer's tube was originally a thermometer (23). I take it in this firſt ſtate, in the inſtance I am going to give. The diſtance between the thermometrical points of melting ice and boiling water, at twenty-ſeven French inches of the barometer, was found to be 1937 parts of a certain ſcale. I broke the bulb of this preparatory thermometer, in a baſon, in order to receive carefully all the mercury that it contained. This being weighed in nice ſcales, amounted to 2 *oz.* 11 *dr.*

 12 *gr.*

12 *gr.* or 1428 grains. All the pieces of my hygrometer being put together, it weighed 373 grains, and when filled with the proper quantity of mercury 833. It consequently contained 460 grains of mercury.

46. By the rule above given (23), the extent of the hygrometer's degrees, ought to be to that of the degrees in the preparatory thermometer, in proportion of the respective weights of mercury in the hygrometer and thermometer; and consequently as the weight of the mercury in the thermometer is to the weight of the mercury in the hygrometer, so is any given interval in the thermometrical scale, to the corresponding interval in the scale of the hygrometer. Consequently in our example as 1428 : 460 :: 1937 : 624 (nearly); and the corresponding intervals on the scales of the thermometer and the hygrometer, ought to follow the proportion of 1937 to 624.

47. I call the distance between the two fixed points of heat in the thermometer the *fundamental interval*; and I shall call the *fundamental line* in the hygrometer that of which the length corresponds to this interval. Thus the *fundamental interval* in the preparatory thermometer, being 1937 parts of a certain scale, the *fundamental line* of my hygrometer consisted of 624 parts of the same scale. This example may so easily be applied, that it will be unnecessary to dwell any longer upon this subject.

48. Having thus got a *fundamental line* in the hygrometer, I had it in my power to divide it into as many parts as I thought proper: my choice was naturally

naturally to be determined by the ſimplicity of a proportion between the degrees of the thermometer, and thoſe of the hygrometer, becauſe this laſt was to be corrected by the firſt, on account of the effects of heat (24). My firſt thought was to divide this *line* into 80 parts, agreeably to the diviſions of the fundamental interval in what I call the *common thermometer* in my book upon the air, which I ſhall always be underſtood to mean in this paper. But as the minuteneſs of theſe degrees was found to be inconvenient and ſuperfluous, I determined to make them double, by putting only 40 in the length of my hygrometer's fundamental line. It is eaſily underſtood that theſe degrees, thus ſettled, begin to be reckoned from the place of the thread, which indicates upon the tube of the hygrometer extreme humidity, by the heat 0 of the *common thermometer*, or of melting ice.

49. The inſtrument with its frame is ſeen fig. 2. the dimenſions of which are every way one half of thoſe of the original. It is mounted on deal, that being the wood, which ſuffers the leaſt change in the length of its fibres. The lower part of the frame is ſlit through the whole length of the ivory pipe, in order that the air may circulate freely round this pipe, and the bulb of a thermometer which I ſhall mention preſently. The hygrometer is faſtened in three parts; viz. at bottom on a ſmall bracket, at top by a tube paſſing through a piece either of hard wood or of metal faſtened by ſcrews; but chiefly by means of a braſs wire on the neck of the braſs piece, which unites the glaſs with the ivory pipe. This piece is laid in a ſmall plate of a hard

wood,

wood, which in that place fills a groove originally made throughout the whole length of the lead-board.

50. To keep the dust from getting through the opening of the tube, I shut it up in a small ivory case. It cannot be sealed up, because if air was left in, it would obstruct the rising of the mercury; and if it was exhausted, the mercury would be pushed to the top by the pressure of the atmosphere upon the ivory pipe; as I have experienced it.

51. Hence however arises a small inconveniency; which is, that as the upper part of the column of mercury communicates with the air, if it remains long in the same part of the tube, or moves but little in it, some dirt may be left on the sides. This I easily remedy, by means of a brass wire, the extremity of which is dentated in the form of a file, in order to hold some bits of cotton, which I put round it. The wire is easily introduced into the tube, by means of the widening mentioned before (35). I put it in, when the mercury is below the part it has soiled, and easily clean it by this means. It is on this account that the tubes to be employed are to be of about $\frac{3}{4}$ of a line internal diameter.

52. The scale of the hygrometer is marked upon a deal slip, which slides along the groove I mentioned before (49). This, as well as all the other parts of the frame, must be lined with paper, to mark the necessary scales; and this paper is afterwards varnished over. Thin plates of silvered brass can be employed for the same use.

53. The mobility of the scale of the hygrometer serves to correct, in the observation itself, the effect

of the heat on the mercury. At the top of this ſcale is ſeen an index, over-againſt another ſmall ſcale, marked upon the unmoveable part of the frame. The degrees of this ſmall ſcale are eightieth parts of the fundamental line, and conſequently immediately anſwer to the degrees of the thermometer on the ſame frame (48). When the index points to o of the ſmall ſcale, the thread which indicates upon the tube of the hygrometer the point to which the mercury ſunk in the melting ice, anſwers likewiſe to o in the ſcale of the hygrometer. This is the caſe expreſſed in the figure wherein the thermometer is likewiſe repreſented at o of its ſcale. By firſt obſerving the heat therefore, and conducting the index to the point of the ſmall ſcale, which anſwers to the actual degree of the thermometer, the hygrometer will only indicate upon its ſcale the degrees of the humor. For this ſcale going through the ſame variations that the heat occaſions in the height of the column of mercury, the indications of the hygrometer become juſt the ſame as they would be, if the heat always continued that of the point at which extreme humidity was fixed, viz. o of the common thermometer.

The ſcale of the hygrometer is carried to the proper point, by means of a knob fixed on a ſmall piece of hard wood or metal, ſcrewed to the bottom of the board, and which affords a free paſſage to the tube of the hygrometer.

Account of the first OBSERVATIONS *made on the going of this* HYGROMETER.

Read June 10, 1773. 54. My first hygrometer was ready for observation at the beginning of last February (1772), in a rainy season. A few hours after it was taken out of melting ice it was already at 54 degrees of its scale. The next morning it was only at 50, but towards noon it rose again to 54. I carried it down to my cellar, which being a considerable depth under ground is commonly very damp. As I went down the stairs, I perceived that my hygrometer continued falling, so that when I hung it up in the cellar it was as low as 35.

55. In the evening of the same day it was at $28\frac{1}{2}$, and the next night at $21\frac{1}{2}$. It continued falling imperceptibly during the rest of this month, throughout the whole of the next, and till the 19th of April. On that day it was at $3\frac{1}{2}$, and consequently very near extreme humidity: but in this interval it had very often rained, and snowed, and even when the sky was clear over head, the streets had always remained wet, so that it was evident from all the common appearances, that the humidity had gone on considerably increasing in the cellar.

56. I was impatient to see the hygrometer rise again in the cellar itself, which I could not however expect but with a north wind. At length, on the 20th of April, though the rain still continued, the hygrometer rose half a degree. In the night of the 20th to the 21st the wind came about to the north, and when I looked at my hygrometer in the morning, I found it at $6\frac{1}{2}$. It continued rising imperceptibly

ceptibly the whole of that day, and the next morning ſtood at $9\frac{1}{2}$.

57. Another circumſtance I was anxious to know, was, whether the hygrometer, after having been kept in the cellar ſo long, would riſe, upon being carried up ſtairs again, to the point from which it had fallen. The importance of this new obſervation prevented me from purſuing that I had begun in the cellar. I therefore took my hygrometer out of it, and while I was going up the ſtairs it roſe three degrees. This was at ſix o'clock in the morning. At ſeven it was already at 17, and at eight at $23\frac{1}{2}$. From eight to eleven it roſe to 43, and at one o'clock ſtood at 63. After this it fell again, and at half an hour after five was no higher than 50. The ſky had been clouded during the laſt interval.

As the preceding obſervations relate only to the hygrometer, and not to humidity, I ſhall confine myſelf to them. They are ſufficient to give an idea of the going of the inſtrument in the ſeaſon they were made. I ſhall hereafter give an account of ſome obſervations taken at other ſeaſons.

FIRST EXPERIMENTS *made to diſcover the Accuracy of this* INSTRUMENT.

58. The moſt important thing after the preceding obſervations, was, to try whether the inſtrument was in reality comparable. To aſcertain this, I immediately conſtructed four more upon the ſame principles, which were finiſhed on the 23d of Auguſt.

59. I could not uſe my firſt hygrometer to make comparative obſervations with the new ones; its tube

 being

being either too narrow or too ſhort. The proportion I had ſettled between the capacity of this tube, and that of the ivory pipe, was deduced from the preliminary experiments I had made in the month of December (29); and had of courſe been found juſt, as long as the ſpring laſted. But even before the new hygrometers were completed, the quickſilver had riſen in the firſt ſo as to run out of the top of the tube. This, joined to ſome other previous obſervations, which had convinced me that the diminution of the humor is much more conſiderable on mountains than in plains (76), induced me to fix the dimenſions of the tube of the hygrometer in the manner laid down in the deſcription of the inſtrument. I had been in time to follow theſe dimenſions in the conſtruction of my new hygrometers, ſo that when they were brought from extreme humidity to the ſtate of the air in my apartment in the month of Auguſt, the quickſilver did not riſe too high in them; that is, it remained ſufficiently below the top of the tube, to indicate leſſer degrees of humidity afterwards.

60. The four new hygrometers have been conſtructed with as little reference to each other, as if they had been made in different countries. By comparing them therefore, I have been enabled to judge of what might be expected from the agreement of inſtruments of this kind. This is what I have found.

When I have obſerved them in places where it appeared likely that the humor would be equally diſtributed among them, the utmoſt of their difference has been uſually from 19 to 21. Their greateſt height, for inſtance, in my room with the windows ſhut,

ſhut, has hitherto been 94, 99½, 100½, 105½, in the ſame moment; which is pretty nearly in the proportion of 19 to 21, between the hygrometer which remains at the loweſt, and that which is at the higheſt.

61. Beſides this difference between the relative altitudes of theſe four inſtruments, I have obſerved another kind of irregularity in them, which is, that they do not always preſerve the ſame proportion to each other. Theſe variations are undoubtedly in part owing to the cauſe itſelf of their motions; that is, to the unequal diſtribution of humor even in places very near each other; but I have reaſon to aſcribe part of them to ſome defect in the inſtruments themſelves. I ſhall hereafter return to theſe cauſes, and give them a cloſer examination.

CONSIDERATIONS *on the Degree of* ACCURACY *that has been obſerved.*

62. Notwithſtanding the defects I have mentioned were rather evident, I was not diſſatisfied with this firſt trial. I never imagined that I had foreſeen every thing, and conſequently could not expect to arrive at a ſufficient degree of exactneſs without the help of experience; the irregularities therefore which appeared in the execution, did not make me deſpair of being able to perfect that inſtrument.

63. My hopes in regard to this were at firſt only grounded upon general reflections. I recollected what the barometer and thermometer had been when they firſt came out of the hands of their inventors; and obſerved that in ſome reſpects they were more

 irregular

irregular than my hygrometer is at present. Though the first of these instruments was very simple in itself, yet barometers hung up in the same places used to vary three or four lines from each other. Some of the members of the French Academy have been themselves engaged in considering a barometer that always kept 18 lines below the rest, and they have formed various hypotheses to account for this difference. The variations therefore of the barometer, though observed only in the same place, were much greater than those between my hygrometers.

64. Nor was the thermometer itself, which is now brought to such a degree of accuracy, much superior at first to our hygroscopes, for the purpose of comparative observations. The first philosophers who treated of it knew nothing of any fixed point or determined degree in it; they knew nothing even of the effect produced by the difference of liquids. In this state of uncertainty the Royal Society adopted the most prudent plan that could be thought of; by giving its sanction to a thermometer to serve as a standard for the construction of those which philosophers should make use of. After this some men of genius endeavoured to establish fixed principles for the making of this instrument. Sir Isaac Newton led the way, but the utility of his first attempts was not sufficiently attended to. Fahrenheit and Reaumur then laboured with great care to settle this point, and we are much indebted to their inquiries. But Fahrenheit's principles were soon rejected, as being too uncertain, though his scale was preserved; and Mr. de Reaumur's, though in appearance admitted for a longer continuance, were in fact so indeterminate,

 that,

that, without perceiving it, a deviation from 80 to 104, was made in the ſpace between the two fundamental points of his thermometer.

65. If in the ſame manner we trace the origin of all inſtruments deſigned for nice menſuration, we ſhould find that have all been defective at firſt, and gradually brought to perfection, when men of genius have thought them worthy of their attention. Thus from the firſt watch, which depended entirely upon the unequal and uncertain action of a ſpring, a ſucceſſion of attempts has produced Mr. Harriſon's valuable time-keeper; and from the firſt balances, which were either too heavy, or too light, we have attained to thoſe ſcales of Mr. Matthey * as eaſily turned as they are accurate. What however is ſtill more aſtoniſhing is, that, notwithſtanding the importance of having fixt meaſures for the dimenſions of bodies, we have not as yet uſed any in practice, but ſuch as muſt be modeled immediately from others.

It is true, that in the conſtruction of an Hygrometer, I was aſſiſted by the general notions of regularity, gathered from the conſtruction of other meaſures of the ſame kind; and of courſe my hygrometer is much forwarder in this reſpect than the thermometer, for inſtance, was in its origin. I therefore only compare the difficulties peculiar to the hygrometer,

* An excellent mechanic, whoſe death is a loſs to a king who knows the value of men of merit. He was a native of Vale-Orbe, in the Pays de Vaud, and in the ſervice of his Sardinian Majeſty; and has written a *Treatiſe on Balances*, which ſerves as a law to all the ſcale makers in the dominions of that prince.

grometer, to thoſe that firſt occurred in all meaſures of phyſical cauſes; and I think that as the latter have been ſurmounted, we ſhould not deſpair of conquering the former. In a word, it is certain that all our inventions only approach towards perfection by degrees, without ever attaining to it entirely; and for this very reaſon, we have a right to expect they will always be drawing nearer and nearer towards it.

Upon theſe notions chiefly, I have raiſed my hopes, either that my hygrometer will in time become more perfect, or at leaſt that it may excite new ideas, which, will at length, though perhaps by ſome other road, lead us to a true meaſure of the humor. As the hope of attaining an end, is one of the moſt powerful aſſiſtants towards really arriving at it, I flatter myſelf at leaſt that I ſhall have given birth to a reaſonable one upon this ſubject.

Firſt Views to improve the HYGROMETER.

66. The idea I entertain, that it is neceſſary a number of attentive men ſhould concur, to improve the human inventions; has induced me firſt to mention the general reaſons I had, for hoping that the hygrometer would be perfected. I ſhall now proceed to give ſome particular reaſons on which this hope is founded, and which are collected from the remarks I have already made upon my inſtrument, during the little time I have had to obſerve it.

The firſt, and one of the moſt important of theſe remarks, is, that the ivory pipe belonging to that hygrometer which is always the higheſt upon its ſcale,

ſcale, happens at the ſame time to be the thinneſt of them all. What connexion there may be between theſe two circumſtances, muſt be determined by experience. But in the mean time it appears to me that if the fibres of the ivory are interwoven with each other; they will make ſo much the leſs reſiſtance either to the being ſeparated or brought cloſer to each other, in proportion as the bundles of theſe fibres have a leſs degree of thickneſs. Whether this remark is of conſequence, or not, we ſhall at leaſt run no riſque in making theſe ivory pipes always exactly of the ſame thickneſs. This indeed was my intention in thoſe which I have made, but unfortunately I thought I ſhould have been able to turn them upon cylinders of hard wood; and found too late, that no accuracy could be expected from this method. It was to remedy this inconvenience, that, in ſpeaking of the manner of turning this piece, I have recommended braſs cylinders (31).

67. The ſame precaution is likewiſe neceſſary to be taken, that we may be certain of giving to every pipe an equal degree of thickneſs throughout the whole of its circumference: a circumſtance no leſs eſſential than the former; ſince I have obſerved in thoſe of my hygrometers whoſe pipes have not an equal thickneſs, that they bend, more or leſs according to the degree of humor to which they are expoſed.

This is probably the principal cauſe why theſe inſtruments do not always preſerve the ſame proportions to each other (58). For the pipes not bending according to the ſame law, there muſt be an irregular

regular change in their capacity, and consequently in the height of the mercury in the tubes. The differences of this kind which I have had occasion to observe, are not indeed very considerable; but, however trifling the cause of an imperfection may be, it is still useful to remove it; were it only to assist us in discovering causes of greater imperfections, by making their effects the more evident.

68. But to make the ivory pipes keep straight, we must attend to a circumstance still more important; which is, that the texture of the ivory be the same in the whole circumference of the pipe. There is a sensible difference in the organization of the external, middle, and internal parts of the same elephant's tooth: nor is it impossible that, besides this difference in the nature, and visible arrangement of its fibres, there may be another arising from their degree of tension; so that some fibres may be more disposed than others to relaxation, after the tooth has been cut to pieces. Suppose then that any of these differences should exist in a pipe, that is, if one of its sides should be more porous, or of a weaker texture, than the other; or if its fibres should be more disposed to relaxation; this pipe will take a bend, either for a constancy, or for a time; and the hygrometers in which it is used, will not of course agree with the others. We must therefore endeavour to make these pipes with a part of the tooth that is homogeneous: that which I believe to be most so, within a certain extent, and which for that reason I have advised, is, the part which is between the center and the surface, and at some inches distance from the apex of the tooth (30).

69. There is another reafon why this different organization of the different parts of the elephant's tooth makes it neceffary to determine exactly the parts that are to be made ufe of in hygrometers. Without this precaution it might happen that the pipes, which ought to be fimilar in every refpect, might be made of fubftances that really differed in their dilatability and fenfibility; that is, of fubftances which the humor might affect more or lefs ftrongly, or more or lefs quickly. This confideration will perhaps oblige us to determine both the fize of the tooth, and the diftance at which the piece ought to be cut off from its apex: for the organization may with equal probability vary in teeth of different thicknefs, and from the apex to the bafe; as it does in the breadth of the fame tooth. I was not fufficiently certain of the fuccefs of my inftrument, to take all thefe precautions when I firft fet about it, but at prefent I believe them to be important.

70. There is ftill another precaution, which indeed I thought neceffary from the beginning, but which I could not manage as I wifhed for want of proper tools; that is, to perforate the ivory pipe exactly in the direction of its fibres. For let the channel have ever fo fmall a degree of obliquity with refpect to this direction of the fibres, thefe fibres will neceffarily be cut in different places; which weakening the pipe where it happens, neither its dilatations nor its contractions can of courfe be regular.

71. I own here are a great number of precautions; but they will not furprize true philofophers. They are accuftomed to obferve the operations of

nature closely; and know that the regularity of her proceedings is connected with a foresight whch is limited to us, by nothing but the limits of our abilities in tracing it; and consequently, when art attempts to imitate nature, it can only succeed in as much as it is attentive to imitate her care.

72. I believe that the hygrometer may farther acquire a perfection of the same kind as that which, in conformity to an idea of my worthy friend Mr. Le Sage, I have given to the thermometer; that is, that we may make its degrees correspond with equal differences in the humor; as I have made those of the thermometer correspond with equal degrees of heat. The way in which I think this might be done, would be to suspend near one of the hygrometers, in a proper vessel which should be placed in one of the scales of a balance that turns very easily, some substance remarkably greedy of the humor; the augmentations or diminutions of weight in which substance, might be compared with the going of the hygrometer, first in the same, and afterwards in different degrees of heat. I hope too, that by a frequent repetition of these observations, at times when the variations of the humor are more or less sudden, we shall at last succeed in correcting the errors that may attend them, from the loss of its own matter the substance made use of may probably suffer by evaporation.

73. These are not the only remarks I have made upon my instrument, but I did not care to mention any but such as have appeared to me most certain. The others are uncertain, and require longer observations. I shall only add therefore, that it will still be

be neceſſary to make freſh experiments, in order to determine the length of time that the ivory pipes ought to remain in water, and how long they muſt afterwards be expoſed to the viciſſitudes of the air (28), or in general to what preparation they muſt be ſubmitted, in order to acquire a laſting degree of conſiſtence before they are made uſe of. For this purpoſe it will alſo be expedient to compare hygrometers recently made, with older ones, both to aſcertain whether they have undergone any alterations, and in what degree. I likewiſe am of opinion, that when we wiſh to fix the point of extreme humidity, we muſt be very careful not to make uſe of any ice but what is very clean, as well internally as externally; leſt any duſt ſhould ſtick to the ivory pipes, which might hinder the water from penetrating into the pores: this is what I thought of myſelf too late. I do not know whether for the ſame reaſon it would not be right to waſh theſe tubes with ſpirits of wine before we put them into the water, to remove any greaſy ſcurf they may have gathered by handling; and afterwards to repeat this at times, in order to carry off any little depoſit of various kinds, which may in courſe of time have been left upon them by the air. Moreover it will be right to inquire whether there is not a difference between the effects of the heat upon the ivory of the hygrometers, and upon the glaſs of the thermometer, ſenſible enough to be attended to, in correcting the effects of this cauſe upon the hygrometer.

74. Having already diſcovered ſo many cauſes, more or leſs probable, of the differences I obſerved

in my hygrometers, I think it reaſonable to hope that this inſtrument will receive a ſenſible degree of perfection on a ſecond trial; and that in time it will be brought to a ſufficient degree of accuracy. It is true there are ſome difficulties in the way of this: but have we not ſufficient motives for endeavouring to overcome them? The air we breathe, and that which ſurrounds us; the places we inhabit, and thoſe which ſerve either to encloſe or to preſerve ſo many different bodies intended for our ſeveral occaſions, are all of them more or leſs filled with that ſubſtance, differently modified, to which I have given the name of *humor*. It alſo produces very ſenſible effects in them; ſome of which very properly excite our curioſity, others may be turned to our advantage, and many of them eſſentially affect our health. It is therefore of great conſequence to natural-philoſophy in general, and to œconomy, and medicine, in particular, that we ſhould obtain a meaſure by which we may, with ſome degree of certainty, eſtimate the local and actual qualities of this ſubſtance, and by this means foreſee its effects; which for the generality we only become acquainted with after they are produced. Theſe ſciences are not perhaps leſs concerned that we ſhould diſcover the nature itſelf of this agent, and the different manners in which it operates: the knowledge of which may enable us to avail ourſelves of reaſon in the inveſtigation of certain effects, which, without ſuch helps, might eſcape our obſervation. As theſe are the ſeveral uſes of an exact hygrometer, we may readily perceive how many new tracks ſuch an inſtrument may open to us, in our inveſtigations of nature, which however

we ſhall not owe to one man alone, but to the joint labours of ſeveral.

Account of ſome of the firſt Phænomena of the HUMOR *obſerved with the* HYGROMETER.

75. Though my firſt advances in this new track of obſervation are as yet very uncertain, yet I will not omit giving ſome account of them. They will at leaſt ſerve to give ſome idea of the going of the inſtrument, as well as of the nature of the agent by which it is governed.

The firſt obſervation I attempted of this kind was with a view to one of the objects which made me deſirous of having an hygrometer. Theſe objects are all comprized in a general ſyſtem concerning vapours, which I have given in my work upon the *Modifications of the Atmoſphere.* I ſhall therefore only mention here one particular conſequence of that ſyſtem, which it was my immediate point to verify; namely, that a certain augmentation of heat, we always perceive at every ſeaſon upon the approach of rain, is owing to a more than ordinary quantity of vapour; and that, on the contrary, it is to their diminiſhed quantity that the leſſer heat of the upper parts of the atmoſphere is in great meaſure to be aſcribed.

76. This latter conſequence was ſupported by an accidental obſervation I made in September, 1770, upon a mountain of the Faucigny, at the height of 1560 toiſes above the level of the ſea. An iron ferule, which ſerved to unite the ends of a cleft ſtick, and which had been fixed on the ſtick with a

hammer

hammer upon the plain, in fine weather, came off of itſelf upon the top of the mountain. When this happened, the thermometer, which I called common, though expoſed to the ſun, was only three degrees above o; while on the plain it was at 18 in the ſhade. This phænomenon, joined with ſeveral others I obſerved at the ſame time, confirmed me in my opinion, that one of the reaſons why the upper parts of the atmoſphere have leſs heat than the lower, is, that they contain leſs humor.

77. With this notion, it became a very intereſting inquiry to know the different degrees of humidity habitual to the different heights of the atmoſphere. Of courſe therefore this was the firſt obſervation I thought of, as ſoon as I had added an hygrometer to the other inſtruments contained within the box of my portable barometer. I undertook therefore to aſcend Buet (the name of that high mountain) a ſecond time. My companions in this expedition were Mr. Dentan, a very intelligent young philoſopher, and my brother, who, having aſſiſted me in all undertakings of any difficulty, had been a witneſs of that fact which was the object of my preſent reſearches.

78. At our ſetting out, on the 29th of laſt Auguſt, the hygrometer was at 86 in my apartment, and the barometer at 27 p. 1 line. We were in hopes of fine weather, becauſe it is generally fair in this country when the barometer at Geneva is above 27 French inches. Soon after we ſet out, we began to perceive that the power of the ſun was greater than might have been expected for the ſeaſon. From this circumſtance I concluded that the barometer muſt fall;

fall, and in fact we found it lower at every place in our way, where we had before obſerved it in fine weather. The ſky, notwithſtanding, was ſtill clear, and continued ſo the next day, when we began to aſcend the mountain, about two o'clock in the afternoon, in order to paſs the night in the higheſt cottages, that we might have more time to gain the ſummit the next day.

79. Before we left Sixt (an abbey at the foot of the mountain), I expoſed the hygrometer in open air, and in the ſhade it ſtood at 94. The thermometer at the ſame time was at 19 in the ſhade, and at 24 in the ſun. At five o'clock we reached a place above 300 toiſes above the abbey; commanded on all ſides by mountains, and on that account called *Les Fonds* (or The Bottoms). Here we obſerved the thermometer and hygrometer. The former, when expoſed to the ſun, ſtood at $15\frac{1}{4}$, and the latter roſe to 96 in the ſhade. We obſerved them again in the ſame manner about half an hour after ſix, in a place that was pretty open, and higher by 160 toiſes than the former. The thermometer ſtood at 15, and the hygrometer at 106. It wanted but a quarter of nine, when we came to the cottages where we were to paſs the night; though they were not above 30 toiſes higher than the place we ſtopped at laſt. The higher we went, the clearer the ſky appeared; in ſo much that, notwithſtanding the uſual augmentation of humor in the air after ſunſet, when the ſky is not clouded, upon expoſing the inſtrument to the air, about $\frac{1}{4}$ after ten at night, we found the hygrometer at 123, and the thermometer at $13\frac{3}{4}$. They both fell in the night, and on our

ſetting

ſetting out the next morning, the former had got down to 109, and the latter to 12.

80. In the two laſt mentioned obſervations the hygrometer had been expoſed long enough to the open air, to conform itſelf to the degree of humor prevalent in the place; but we had not time for the obſervations I was moſt deſirous to make with accuracy. The hygrometer being uſually ſhut up in the box of my barometer, it would have been neceſſary to have left that open ſome time, in order that it ſhould adapt itſelf to the ſtate of the air, and we could allow but a very ſhort time for theſe obſervations.

81. The firſt of them was made at nine in the morning, at the height of about 1000 toiſes above the plain. The ſky appeared clear over head, but the plain was darkened with vapours. The thermometer in the ſun ſtood at 13¼, and the hygrometer roſe to 115 in the ſhade.

82. It was two in the afternoon when we reached the top of the mountain, which is always covered with an enormous maſs of ice and ſnow. We found there a very ſtrong ſouth wind, which is the warmeſt wind in our plains: beſides this, we were nearly at the hotteſt time of the day; and yet the thermometer, upon being expoſed to the ſun, ſhewed only 6. The wind, and the coldneſs of this region, obliged us to quit the ſummit in a quarter of an hour, during which the hygrometer had riſen only to 119; but we judged that it was not yet ſtationary.

83. In this ſhort time we experienced a new effect of the diminiſhed humidity of the air, which ſurprized us all three very much. We found our ſkin withered

withered and pale, ſo that both to the ſight and to the touch, it reſembled much a dry and ſhriveled bladder. Notwithſtanding this we were ſenſible of no other inconvenience but what aroſe from the wind and the cold: the action of the lungs and the functions of all the other parts of the body were perfectly free, though the barometer was only at 19 inches, 6 lines and a half.

84. We quitted the ſummit at about a quarter after two, to ſhelter ourſelves from the wind behind ſome rocks, which were nearly 50 toiſes lower. Here we ſtayed about an hour. During this time the hygrometer, expoſed to the air, but always in the ſhade, roſe by imperceptible degrees to $132\frac{1}{4}$. It would probably have riſen higher, had not we been obliged to quit this place, where the clouds began to gather, in order to reach the cottages before night. It was indeed already too late before we thought of retiring; for we were overtaken by the night, and a thunder ſtorm, at a ſufficient diſtance from our hut to expoſe us to the greateſt danger of being loſt, notwithſtanding our guides, but for the aſſiſtance of two women, whoſe humanity deſerves the higheſt commendations. Theſe women, who lived in our cottages, being apprized of our diſtreſs by our cries, notwithſtanding the ſtorm, and the ſcarcity of wood in theſe places, came out to kindle a great fire at the foot of the rocks on which we were wandering amidſt the precipices, in total darkneſs; and ſometimes with great difficulty keeping the fire alive, ſometimes advancing towards us with fire-brands till the wind and rain extinguiſhed them, and endeavoured, with the moſt unaffected concern, to point out to us the

path we ought to keep. At laſt, animated by the courage of theſe women, directed partly by their light and partly by their cries, we at length reached the cottage, much more affected with the humanity of theſe good people, than hurt with the dangers and fatigues we had undergone.

85. The ſtorm laſted a great part of the night, and it rained almoſt without intermiſſion. Notwithſtanding this, the hygrometer, when expoſed to the air the next morning, ſtood at 105, and the thermometer at 10. As we were uncertain how long the rain would continue, we ſet out at eight in the morning on our way down. The rain hardly ceaſed the whole morning, and was ſometimes accompanied with hail; it ſtill continued raining when we arrived at the abbey about noon, notwithſtanding the hygrometer ſtood there at 99, that is to ſay, five degrees higher than when we ſet out; but the barometer, which had fallen the two preceding days, was now beginning to riſe; the thermometer was at 14.

86. We learnt at Sixt, that, at the very time we were driven from the ſummit of the mountain by the diſagreeable coldneſs of the air, they had felt an exceſſive degree of heat, and likewiſe that the ſtorm had been very violent in the night. This ſtorm, as we found two days after at Geneva, had extended itſelf all over the plain. We found likewiſe, from the obſervations that had been made there in our abſence, that a thermometer expoſed to the north, conſequently out of the ſun, had been at 23½, at the very time that ours, at the top of the mountain and in the ſun, had been only at 6.

87. As,

87. As, in mentioning the particular purport of the foregoing obſervations, I have not explained my ſyſtem concerning vapours, I ſhall not here ſtop to draw the conſequences that may be deduced from them in favour of this ſyſtem. Indeed, to ſay the truth, I think them too few and too imperfect to conclude any thing from them as yet. I have only related them, as I declared at firſt, to give a general notion, both of the going of my hygrometer, and of the inquiries that may be purſued with its aſſiſtance. It is with the ſame intention that I proceed to relate ſome obſervations of another kind.

88. Some accidental obſervations had made me ſuſpect that the immediate action of the ſun upon my hygrometer produced a drying, which might not be wholly occaſioned by the real ſtate of the air with reſpect to the humor, but might depend in ſome meaſure upon ſome ſingular property of the ſolar rays, which we ſee produce effects upon ſome bodies, not immediately to be accounted for by the ordinary laws of heat. This firſt remark induced me, as I have taken care to mention, always to obſerve the hygrometer in the ſhade upon the mountains of Sixt. At my return, I determined to examine more accurately whether my conjecture in this reſpect had any foundation.

89. The firſt thought that occurred to me for this purpoſe was, to obſerve two hygrometers at the ſame time, one in the ſhade and the other in the ſun, very near each other, that the ſame air might circulate freely round them. The air of the country having appeared to me more proper for this obſervation than

A TABLE of OBSERVATIONS made on the 13th of September, on two HYGROMETERS, the one in the Shade, and the other in the Sun, each of them accompanied with a THERMOMETER.

	Hour of the Day	Therm. in the Shade.	Hygrom in the Shade.	Hygrom. in the Sun.	Ther. in the Sun.
The Bar. at 27 Fr. inch. 1 line. The sun did not shine yet on that part of the garden.	7	8	29	36½	8
The sun has now shone for ¼ of an hour on the Hygr. and Therm. which are to stand exposed to it.	7½	11¾	36½	66½	12
	8	12¼	43½	82	12½
	9	13	67	102	13½
	10	14½	76½	109	15½
	11	15	87½	116	16¼
	Noon	15½	96½	120½	17¼
	1	16¾	103	126	18
The vapours condensing in the air weaken the action of the sun.	2	16¼	103	125	17¼
Barom. 27. inch. A South wind begins to blow.	3	16¾	102¼	123	17¼
The clouds rise.	4	15¾	107	133	16
The clouds meet, and the sunshine withdrawn.	5	13¼	88½	106	13¼
The sun is set, and the weather quite overcast.	6	12	64½	81	12
Barom. 26 inches 11 lines. . .	7	11¼	50	65	11¼
	8	11	37	50	11
	9	10¾	31	41	10¾
The clouds break, and the dew begins to appear on the plants.	10	10½	24	35	10½
	11	10	20½	26½	10
The clouds meet again. . . .	Mid.	10½	24½	28½	10½
	1	11¼	23	27	11¼
It begins to rain.	2	11¼	27	22	11¼

99. The

92. The firſt circumſtance in theſe obſervations that deſerves to be noticed, is the difference in the ſinking of the two hygrometers when they were expoſed to the air, before the ſun ſhone in the garden. They both of them fell conſiderably, but one of the two 7 degrees and a half leſs than the other. One of the cauſes of this diſparity is probably in the inſtruments themſelves, and is owing to their being differently affected by the action of the humor. There is a difference of the ſame kind obſervable in the thermometers, which are likewiſe more or leſs ſenſible to the impreſſions of the heat even when the bulk of their liquid is the ſame; that is to ſay, they are acted upon more or leſs quickly by the degree of heat which ſurrounds them, according to the thickneſs, or even according to the nature of the glaſs of which the ball is made. Conſequently it is poſſible that the different thickneſs or poroſity of the ivory may have had ſome influence on the going of the hygrometer in this obſervation (66 and 69).

93. But theſe differences in the ivory pipes muſt produce a much greater difference in the ſenſibility of the hygrometers, than thoſe of the glaſs balls can produce in the thermometers; becauſe it is much more difficult for the humor to penetrate the ivory, than for the heat to get through the glaſs. So that any encreaſe of the obſtacles retards the introduction of the humor, much more than that of the heat; and conſequently the difference of ſenſibility muſt be more difficult to be prevented in the hygrometers, than it is in the thermometers.

This ſlowneſs of the humor in pervading the bodies into which it inſinuates itſelf, makes it a de-

ſirable circumſtance, that the ivory pipe of the hygrometer ſhould be the thinneſt poſſible; in order that it might be more readily affected. This I had foreſeen, before I had learnt it from experience; but I was afraid of its being attended with ſtill greater inconveniences than that it was intended to remedy; from the action of the mercury againſt pipes whoſe ſides would be thinner. However, this might be tried. In the mean time, I fancy that, for obſervations in which it is abſolutely neceſſary that the inſtrument ſhould eaſily be affected, leſſer hygrometers might be made, whoſe tubes containing a leſs quantity of mercury, would reſiſt the action of it, though with a leſs degree of thickneſs. (Perhaps it would not be impoſſible to uſe tubes made of ſome very thin quills.) I cannot yet aſcertain whether theſe little hygrometers could be graduated by themſelves, or whether they muſt be compared with thoſe of which I have given the dimenſions; this we ſhall learn from experience.

94. The difference there is between the heat and the diſcrete humor in the power of diffuſing itſelf, occaſions in another reſpect a conſiderable difference in the goings of the thermometer and hygrometer. The heat is brought into a ſtate of equilibrium much ſooner and with much greater certainty than the humor. Two thermometers accurately conſtructed and fixed near each other, in a place where the heat does not change very ſuddenly, always agree together. This is not the caſe with two hygrometers: they ſeldom agree, that is, they ſeldom preſerve the ſame conformity to each other, when there is the leaſt variation in the humor: at ſome times their difference increaſes, at others it diminiſhes; this can only

only arife from a difference in the caufe that acts upon them.

95. We may form our ideas of the manner in which the invifible humor diftributes itfelf; from that in which all kinds of vifible vapours are diffufed. We fee them feparate, re-unite, fly off from certain places, rufh into others, and in fhort yield to every impreffion of the air. The motion peculiar to their own particles, which I look upon as the caufe of their elafticity *, is not fufficiently rapid, and the vapours themfelves are too thick to overcome always the contrary motion of the air. This, I believe, is what conftitutes the chief difference between vapours, and the igneous fluid, as far as relates to the power of putting themfelves into a ftate of equilibrium in the air, which is moving. The current of air carried towards a chimney which has fire in it, frees the room from fmoke, and is but a very flight impediment to the diffufion of the heat through it.

96. Though the invifible vapours by reafon of their exceffive thinnefs are more capable of being put in equilibrium in the air than the vifible ones, they are very far from having this property in as great a degree as the heat. Which leads me to think, that part of the difference obferved between my hygrometers, even before funrife, may have been owing to the unequal diftribution of the humor, though the two inftruments were only at the diftance of a foot from each other, without the interpofition of any folid body.

* The fyftem I adopted on that point may be found in my work upon the *Modifications of the atmofphere.*

97. I ſhall not attribute intirely to the ſame cauſe, the great difference obſerved between my hygrometers, when one was expoſed to the ſun, while the other ſtood in the ſhade. The immediate action of the ſolar rays, or of the luminous heat, produces a variety of effects, which, as I have ſaid before, do not appear to follow the ſame laws as thoſe of dark heat. And if I may be allowed to propoſe a conjecture upon this particular point, before fuller experiments have been made, it ſhould ſeem, that the immediate action of the ſolar rays muſt occaſion a greater evaporation than what is produced by dark heat, even when they hold the thermometer at the ſame height. But let the cauſe be as it will, we ſee by this experiment, that in a ſection of air about a foot wide, through which the ſolar rays did not immediately paſs, the action of the humor upon the hygrometer was 23 degrees greater than in the place round about; though that of the heat upon the thermometers was only a degree and a quarter leſs; which leads us to conceive how many apparently ſmall cauſes may contribute to produce ſenſible differences in the diſtribution of the diſcrete humor.

98. Another uſe to be made of theſe obſervations is, to compare them with thoſe that I have made in the mountains of Sixt; in order to form a better judgment of the proportion between the different degrees of humidity, in the ſuperior and inferior parts of the atmoſphere. My hygrometer, held in the ſhade upon the ſummit of Buet, roſe to $132\frac{1}{2}$, and was not yet ſtationary. This is pretty nearly the greateſt degree of dryneſs obſerved in the hygrometer expoſed to the ſun in the garden; while the

the one that remained in the ſhade, the ſame upon which the obſervation at the mountain had been made, was not in fact higher than 103, though marked in the table of obſervations at 107 (91)

99. But the difference between the obſervations made upon the mountain of Sixt, and thoſe I am ſpeaking of, was ſtill greater by much after ſun-ſet. The 30th of Auguſt, at a quarter after ten at night, I obſerved the hygrometer without ſide the cottage upon the mountain, and found it at 123 (79); and on the 13th of September following, in the plain, it was not higher than 31 at 9, and 24 at 10 o'clock. The wind was ſouth, and the height of the barometer upon the plain, pretty much the ſame during both the obſervations.

100. It is true that, notwithſtanding the ſimilarity of theſe circumſtances, theſe obſervations cannot be directly compared, on account of the diſagreement in ſome other circumſtances. In the firſt place, the difference of fourteen days at this ſeaſon of the year may have produced a ſenſible change in the ſtate of the air. There was already, for inſtance, a conſiderable difference in the degrees of the thermometer; it was at 13 and ¾ when the obſervation was made on the mountain, and no higher than at 10 on the plain. Beſides, at this time of night, there would always be an eſſential difference between the upper and lower parts of the atmoſphere, even though in the day time they ſhould have the ſame degree of humidity: for the vapours being condenſed after ſun-ſet, and thus producing a kind of dew, they muſt neceſſarily deſcend, and from this very cauſe be more abundant in the low grounds than

on the higher ones. I ſhall add, that though my hygrometer was expoſed to the open air on the mountain, as it was in the plain, yet it was not ſo much inſulated there, being tied to the box of my portable barometer. The difference obſerved, however, is ſo conſiderable, that, notwithſtanding the concurrence of all theſe particular cauſes, I cannot but aſcribe it in ſome meaſure to that general one which I have ſuſpected, namely, that there is comparatively a leſs degree of humidity in the upper than in the lower parts of the atmoſphere.

101. The obſervation of the 13th of September ſeems likewiſe to throw ſome light upon the phænomena of dew. We know that when the ſky is cloudy, there is little or no dew, and it has likewiſe been obſerved from this very circumſtance, that the air is not ſo much cooled after ſun-ſet. The cauſe of theſe differences appears to me to be, that when there are no clouds in the air at ſun-ſet, or when they are diſperſed, the heat of the inferior air, and that which riſes from the earth, diſſipates itſelf into the ſuperior regions, and then the vapours which are diſperſed throughout the air condenſe and fall down again in dew; but when the clouds are continued, and thus ſeparate the inferior from the ſuperior air, they prevent this diſſipation of the heat, and the vapours remain ſuſpended. And if the ſky grows cloudy ſome hours after the ſetting of the ſun, and after the heat has ſenſibly diminiſhed in the inferior air, it encreaſes again in it; becauſe the heat, which continues to riſe out of the earth, is accumulated in the inferior air. This appears in the obſervation I am ſpeaking of. The clouds having been ſepa-

rated for a while, at 10 o'clock there was ſome dew, and the hygrometer fell ſenſibly till eleven: but afterwards the clouds cloſing again, the heat encreaſed, and the humidity evidently diminiſhed.

102. I take it for granted here, that the moſt common and moſt plentiful dew proceeds from the air, and not from the earth, as ſome philoſophers have imagined. I ſhould produce the proofs I have collected of this fact from a multitude of experiments, if it had not been done in an excellent paper, written by Profeſſor le Roi, *On the elevation and ſuſpenſion of water in the air* *. Theſe phænomena of the dew become very intereſting examined with the help of the hygrometer, and joined to obſervations of the degrees of ſaturation of the air with reſpect to water, which have been ſo ingeniouſly imagined, and begun by the author of this memoir. If this part of natural philoſophy is ever cleared up, as I hope it will be, we ſhall be much indebted for it to the ſagacity of this true philoſopher.

103. I ſhall only mention one more obſervation I have endeavoured to make with my hygrometer, which ought not to be omitted, as it is connected with the principles upon which the inſtrument is conſtructed. It has likewiſe a reference to medicine, in as much as one of the objects of that ſcience, in its inquiries to preſerve our health, is to determine the effects of water at different degrees of heat upon our organs. Ivory being an animal ſubſtance, the effects produced upon it by water at different degrees

* Mem. de l'Ac. des Sc. de Paris, for the year 1751.

of

of heat, may aſſiſt us in diſcovering thoſe which are produced upon our bodies from the ſame cauſe.

104. The point o of my hygrometer, as I have before obſerved (44), is that of the extreme humidity produced by melting ice. It was therefore of ſome importance to know what difference there would be in this point, when the hygrometer ſhould be plunged into warmer water. This I endeavoured to find out; and the following is the reſult of my firſt inquiries.

105. The moment I took one of my hygrometers out of melting ice, I plunged it into water at the heat of 45 degrees of the thermometer that I have called common. It fell ſuddenly four of its degrees below the thread which marked its height in the melting ice, but immediately roſe again, and in four minutes reached 8 degrees and a half above the ſame thread. Deducting $22\frac{1}{2}$ from the height for the dilatation of the mercury (48), there will remain 14. Conſequently the water warmed at 45 degrees of the common thermometer, really made the hygrometer ſink 14 degrees below o.

106. Half an hour after this, the water being at 38 degrees, I found the hygrometer no higher than $6\frac{1}{2}$, that is to ſay, $6\frac{1}{2} - \frac{38}{2} = -12\frac{1}{2}$. Conſequently the true point of humidity indicated by the hygrometer was $12\frac{1}{2}$ below o. Laſtly, the heat of the water being reduced to 28 degrees the hygrometer was at $3 - \frac{28}{2} = -11$. I was then obliged to put an end to the experiment, which I have not been able to take up again ſince, for want of leiſure. But

 what

what has already been obferved is fufficient to fhew us that the warmer the water is, the more it dilates the ivory (though we faw that the mercury rofe in the hygrometer after having funk for a moment). From hence, I fancy, may be drawn this general confequence, already indeed forefeen, namely, that in *an equal acting quantity*, the warmer the humor is, the more it feparates the particles of thofe bodies which it pervades.

107. I fay, in an *equal acting quantity*; and this is one of the objects which will probably furnifh us with a variety of moft ufeful knowledge, at the fame time that it is moft likely to give the greateft exercife to the genius and attention of natural philofophers.. The forementioned experiment proves, that the warmer the water is, the more it dilatates the ivory pipe of the hygrometer, and the fame thing I make no doubt happens with the difcrete humor.

On the other hand, the evaporation being certainly greater in fummer than in winter, there muft of courfe be more vapours in the air in the firft of thefe feafons than in the latter. Thefe then, as it appears, are the two circumftances moft likely to make the hygrometer fall in fummer; a greater degree of humor in the air, and an encreafe of heat. And yet I have already experienced that the mean height of the hygrometer is greater in fummer than in the other feafons. I found my firft hygrometer, which was made in winter, too fhort in the fummer; but it would be of a fufficient length now that we are in autumn. The mean height of the four new ones is already (the beginning of November)

ber) 17 degrees leſs than it was in the months of Auguſt and September.

108. I hope this paradox will be explained, and that the principles which may clear it up will draw uſeful conſequences along with them. Thoſe philoſophers who look upon evaporation as a diſſolution of water by air in the manner of menſtrua, that is, by affinity, will eaſily apply their principle to the ſolution of part of theſe phænomena. The diſſolution is greater when the menſtruum is warmer, and conſequently the air muſt keep a greater quantity of water in diſſolution, and ſuffer a leſs part of it to be precipitated, in ſummer than in winter. I cannot but allow that this ſyſtem is extremely ſpecious, and that many phænomena are very happily explained by means of it. This is what Mr. le Roy has ſhewn us in the memoir I have already quoted; in which, without contending that air really acts as a menſtruum with reſpect to water, he demonſtrates, by a parallel very well kept up, that all the chemical expreſſions concerning diſſolutions may with propriety be applied to deſcribe the ſeveral phænomena he examines, relative to the elevation and ſuſpenſion of water in air, as well as to its precipitation under different forms.

109. If it was not too common a practice, to conclude things from words, I ſhould in fact think theſe chemical expreſſions very conveniently adapted to explain a number of theſe phænomena. But I have rejected them here, on account of this conſideration; that when I took in a greater number of phænomena, I found them no longer accurate, any more than the general idea of the diſſolution

tion of water by air. I have given the reaſons for this in my work, upon the modification of the atmoſphere; and ſhall only repeat here, that theſe modifications of the humor appear to me almoſt intirely to be produced by the igneous fluid; and that if the air has any ſhare in them, it is only as being an elaſtic fluid. The particles of theſe fluids, each according to its degree of power, ſtrike, ſeparate, and draw along with them thoſe of the humor, and communicate to them the elaſticity they poſſeſs; in the ſame manner as they do to the particles of all volatile, and likewiſe of all fixt ſubſtances which they corrode and decompoſe.

110. This ſyſtem will not only furniſh a ſolution of the paradox which engages our attention, but will, I believe, carry us much farther. The heat of the ſummer keeps the humor in very great agitation; and though there is more of the humor at this ſeaſon than in winter, yet this heat will not allow it to continue either as long a time, or in as great a quantity, upon the bodies or in their pores. That is the reaſon why the hygrometer falls leſs. But we ſee at the ſame time, that the portion of the humor which does ſojourn, and which I call the active part, has more power to dilatate the bodies, from the greater degree of motion impreſſed upon it by a greater heat. Conſequently the dilatation of the bodies, from this cauſe, will be in a compound ratio of the quantity of humor, and of its active force, or of the heat. And if, for inſtance, we compare any ſummer's day, in which the hygrometer in open air is at the ſame degree as on any winter's day, the air on the ſummer's day will contain more humor than on the

 winter's

winter's day; but there will be leſs of it will act upon the hygrometer; and yet as the active part will have more ſtrength, the effect upon the whole will be the ſame. This is what appears to me, but I can not now enlarge any farther on this ſyſtem. I have ſaid enough to ſhew that the ſubject is very expenſive, and deſerves an attentive examination.

Fig: II.

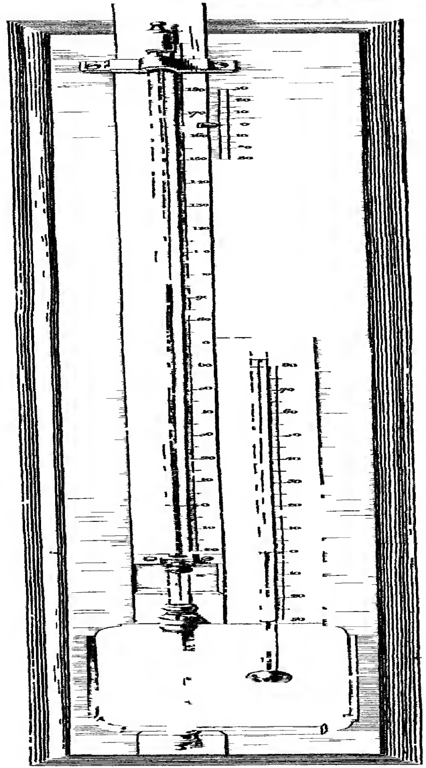
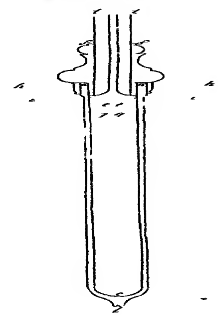


Fig: I.



Hygrometer

J. Bruce sculp.

XXXIX. *Of the electric Property of the Torpedo. In a Letter from* John Walsh, *Esq; F. R. S. to* Benjamin Franklin, *Esq; LL. D., F. R. S., Ac. R.* Par. *Soc. Ext.*, &c.

Chesterfield-Street, July 1, 1773.

DEAR SIR,

Read July 1, 1773. I Am concerned that other engagements have prevented me from giving to the Royal Society, before their recess, a complete account of my experiments on the electricity of the Torpedo; a subject not only curious in itself, but opening a large field for interesting inquiry, both to the electrician in his walk of physics, and to all who consider, particularly or generally, the animal œconomy.

To supply the deficiency in the best manner I am now able, I will request the favour of you to lay before the Society my letter from la Rochelle, of the 12th July 1772, and such part of the letter I afterwards wrote from Paris, as relates to this subject. Loose and imperfect as these informations are, for they were never intended for the public eye, they are still the most authentic, and so far the most satis-

ſatisfactory I can at preſent offer, ſince the notes I made of the experiments themſelves remain nearly, I am ſorry to ſay it, in that crude and bulky ſtate in which you had the trouble to read them.

Letter from Mr. WALSH to Dr. FRANKLIN, dated la Rochelle, 12th July 1772.

" It is with particular ſatisfaction I make to you " my firſt communication, that the effect of the " Torpedo appears to be abſolutely electrical; by " forming its circuit through the ſame conductors " with electricity, for inſtance metals, and water; " and by being intercepted by the ſame non-con- " ductors, for inſtance glaſs, and ſealing-wax. I " will not at preſent trouble you with the detail of " our experiments, eſpecially as we are daily ad- " vancing in them; but only obſerve, that we have " diſcovered the back and breaſt of the animal to be " in different ſtates of electricity: I mean in par- " ticular the upper and lower ſurfaces of thoſe two " aſſemblages of pliant cylinders, of which you " have ſeen engravings in Lorenzini *. By the " knowledge of this circumſtance we have been " able to direct his ſhocks, though they were very " ſmall, through a circuit of four perſons, all feel- " ing them; likewiſe through a conſiderable length " of wire held by two inſulated perſons, one touching " his lower ſurface, and the other his upper. When

* Oſſervazioni intorno alle Torpedini di Stef. Lorenzini 1678.

Redi appears to be the firſt who remarked theſe ſingular parts of the Torpedo in 1666. Franc. Redi, Exper. Nat.

" the

"the wire was exchanged for glass, or sealing-wax, "no effect could be obtained: but as soon as it was "resumed, the two persons became liable to the "shock. These experiments have been varied many "ways, and repeated times without number, and "they all determined the choice of conductors "to be the same in the Torpedo as in the Leyden Phial. The sensations likewise, occasioned "by the one and the other in the human frame, are "precisely similar. Not only the shock, but the "numbing sensation which the animal sometimes "dispenses, expressed in French by the words *engourdissement* and *fourmillement*, may be exactly imitated with the Phial, by means of Lane's Electrometer; the regulating rod of which, to produce the "latter effect, must be brought almost into contact "with the prime conductor which joins the Phial. "We have not yet perceived any spark to accompany "the shock, nor the pith balls to be ever affected. "Indeed all our trials have been on very feeble "subjects, whose shock was seldom sensible beyond "the touching finger: I remember but one, of at "least two hundred, that I myself must have received, to have extended above the elbow. Perhaps the Isle of Ké, which we are about to visit, "may furnish us with Torpedos fresher taken and of "more vigour, by which a farther insight into these "matters may be had. Our experiments have been "chiefly in the air, where the animal was more "open to our examination than in water. It is a "singularity that the Torpedo, when insulated, "should be able to give to us, insulated likewise, "forty or fifty successive shocks from nearly the

 "same

" fame part; and thefe with little, if any diminu-
" tion in their force: indeed they were all very
" minute. Each effort in the animal to give the
" fhock is conveniently accompanied with a de-
" preffion of his eyes, by which even his attempts
" to give it to non-conductors can be obferved.
" The animal, with refpect to the reft of his body,
" is in a great degree motionlefs, but not wholly fo.
" You will pleafe to acquaint Dr. Bancroft, of our
" having thus verified his fufpicion concerning the
" Torpedo *, and make any other communication
" of this matter you may judge proper. Here I
" fhall be glad to excite, as far as I am able, both
" electricians and naturalifts, to pufh their inquiries
" concerning this extraordinary animal, whilft the
" fummer affords them the opportunity."

Extracts of a Letter from Mr. WALSH to Dr. FRANKLIN, dated Paris, 27th Auguft, 1772.

——— " I fpent a complete week in my ex-
" periments at the Ifle of Ré, and had there every
" convenience for profecuting them to their extent,
" except that I was reftrained by the jealoufy of
" the government from making them where the
" animal was caught. At my return to la Ro-
" chelle, I communicated to the members of the
" Academy of that place, and to many of the prin-
" cipal inhabitants, all that I had obferved con-
" cerning the Torpedo, in the intention of ftirring

* Bancroft's Natural Hiftory of Guiana, p. 194.

" up

" up a ſpirit of inquiry, both as to its electricity and
" general œconomy.

——— " The vigour of the freſh taken Torpedos at the Iſle of Ré, was not able to force the torpedinal fluid acroſs the minuteſt tract of air; not from one link of a ſmall chain, ſuſpended freely, to another; not through an almoſt inviſible ſeparation, made by the edge of a penknife, in a ſlip of tinfoil paſted on ſealing-wax. The ſpark therefore (of courſe the attendant ſnapping noiſe) was denied to all our attempts to diſcover it, not only in day-light, but in complete darkneſs. I obſerved to you, in my laſt, the ſingularity of the Torpedo being able, when inſulated, to give to an inſulated perſon a great number of ſucceſſive ſhocks: in this ſituation I have taken no leſs than fifty from him in the ſpace of a minute and an half. All our experiments confirmed, that his electricity was condenſed, in the inſtant of its exploſion, by a ſudden energy of the animal; and as there was no gradual accumulation, nor retention of it, as in the caſe of charged glaſs, it is not at all ſurprizing that no ſigns of attraction or repulſion were perceived in the pith balls. In ſhort, the effect of the Torpedo appears to ariſe from a compreſſed elaſtic fluid, reſtoring itſelf to its equilibrium in the ſame way, and by the ſame mediums, as the elaſtic fluid compreſſed in charged glaſs. The ſkin of the animal, bad conductor as it is, ſeems to be a better conductor of his electricity, than the thineſt plate of elaſtic air. Notwithſtanding the weak ſpring of the torpedinal electricity, I was

" able

" able, in the public exhibitions of my experiments " at la Rochelle, to convey it through a circuit, " formed from one ſurface of the animal to the " other, by two long braſs wires, and four perſons, " which number, at times, was increaſed even to " eight. The ſeveral perſons were made to com- " municate with each other, and the two outermoſt " with the wires, by means of water contained in " baſins, properly diſpoſed between them for the " purpoſe; each perſon dipping his hands in the " neareſt baſins, connectively with his neigbour on " either ſide.——

" The effect produced by the Torpedo, when " in air, appeared, on many repeated experiments, " to be about four times as ſtrong as when in " water."

A clear and ſuccinct narrative of what paſſed at one of the public exhibitions, alluded to in the laſt letter, appeared in the French Gazette of the 30th October 1772. As it came from a very reſpectable quarter, not leſs ſo from the private character of the gentleman, than from the public offices he held, I muſt deſire leave of the Society to avail myſelf of ſuch a teſtimony to the facts I have advanced, by giving a tranſlation of that narrative.

Extract of a Letter from the Sieur SEIGNETTE, Mayor of la Rochelle, and ſecond perpetual Secretary of the Academy of that City, to the publiſher of the French Gazette.

" In the Gazette of the 14th Auguſt, you men- " tioned the diſcovery made by Mr. Walſh, Mem- " ber

" ber of the parliament of England, and of the
" Royal Society of London. The experiment, of
" which I am going to give you an account, was
" made in the presence of the Academy of this
" city. A live Torpedo was placed on a table.
" Round another table stood five persons insulated.
" Two brass wires, each thirteen feet long, were
" suspended to the ceiling by silken strings. One
" of these wires rested by one end on the wet
" napkin on which the fish lay; the other end
" was immersed in a basin full of water placed
" on the second table, on which stood four other
" basins likewise full of water. The first person
" put a finger of one hand in the basin in which
" the wire was immersed, and a finger of the
" other hand in the second basin. The second
" person put a finger of one hand in this last
" basin, and a finger of the other hand in the
" third; and so on successively, till the five per-
" sons communicated with one another by the
" water in the basins. In the last basin one end
" of the second wire was immersed; and with
" the other end Mr. Walsh touched the back of
" the Torpedo, when the five persons felt a com-
" motion which differed in nothing from that of
" the Leyden experiment, except in the degree of
" force. Mr. Walsh, who was not in the circle
" of conduction, received no shock. This expe-
" riment was repeated several times, even with
" eight persons; and always with the same suc-
" cess. The action of the Torpedo is commu-
" nicated by the same mediums as that of the
" electric fluid. The bodies which intercept the

 " action

" action of the one, intercept likewise the action
" of the other. The effects produced by the
" Torpedo resemble in every respect a weak elec-
" tricity."

This exhibition of the electric powers of the Torpedo, before the Academy of La Rochelle, was at a meeting, held for the purpose in my apartments, on the 22d July 1772, and stands registered in the journals of the Academy.

The effect of the animal was, in these experiments, transmitted through as great an extent and variety of conductors as almost at any time we had been able to obtain it, and the experiments included, nearly, all the points, in which its analogy with the effect of the Leyden Phial had been observed. These points were stated to the gentlemen present, as were the circumstances in which the two effects appeared to vary. It was likewise represented to them; That our experiments had been, almost wholly with the animal in air: That its action in water was a capital desideratum: That indeed all as yet done was little more than opening the door to inquiry: That much remained to be examined by the Electrician as well as by the Anatomist: That as artificial electricity had thrown light on the natural operation of the Torpedo, this might in return, if well considered, throw light on artificial electricity, particularly in those respects in which they now seemed to differ: That for me, I was about to take leave of the animal, as nature had denied it to the British seas; and that the prosecution of these researches

rested in a particular manner with them, whose shores abounded with it.

The Torpedo, on this occasion, dispensed only the distinct, instantaneous stroke, so well known by the name of the electric shock. That protracted but lighter sensation, that Torpor or Numbness which he at times induces, and from which he takes his name, was not then experienced from the animal; but it was imitated with artificial electricity, and shewn to be producible by a quick consecution of minute shocks. This, in the Torpedo, may perhaps be effected by the successive discharge of his numerous cylinders, in the nature of a running fire of musketry: the strong single shock may be his general volley. In the continued effect, as well as in the instantaneous, his eyes, usually prominent, are withdrawn into their sockets.

The same experiments, performed with the same Torpedos, were on the two succeeding days, repeated before numerous companies of the principal inhabitants of La Rochelle. Besides the pleasure of gratifying the curiosity of such as entertained any on the subject, and the desire I had to excite a prosecution of the inquiry, I certainly wished to give all possible notoriety to facts, which might otherwise be deemed improbable, perhaps by some of the first rank in science. Great authorities had given a sanction to other solutions of the phoenomena of the Torpedo; and even the Electrician might not readily listen to assertions, which seemed, in some respects, to combat the general principles of electricity. I had reason to

 make

make ſuch concluſions from different converſations I had held on the ſubject with eminent perſons both at London and Paris. It is but juſtice to ſay, that of all in that claſs you gave me the greateſt encouragement to look for ſucceſs in this reſearch, and even aſſiſted me in forming hypotheſes, how the Torpedo, ſuppoſed to be endued with electric properties, might uſe them in ſo conducting an element as water.

After generally recommending to others an examination of the electric powers of theſe animals when acting in water, I determined, before I took my final leave of them, to make ſome farther experiments myſelf with that particular view; ſince, notwithſtanding the familiarity in which we may be ſaid to have lived with them for near a month, we had never detected them in the immediate exerciſe of their electric faculties againſt other fiſh, confined with them in the ſame water, either in the circumſtance of attacking their prey, or defending themſelves from annoyance: and yet that they poſſeſſed ſuch a power, and exerciſed it in a ſtate of liberty, could not be doubted.

A large Torpedo, very liberal of his ſhocks, being held with both hands by his electric organs above and below, was briſkly plunged into water to the depth of a foot, and inſtantly raiſed an equal height into air; and was thus continually plunged and raiſed, as quick as poſſible, for the ſpace of a minute. In the inſtant his lower ſurface touched the water in his deſcent, he always gave a violent ſhock, and another ſtill more violent in the inſtant of quitting the water in his aſcent;

aſcent; both which ſhocks, but particularly the laſt, were accompanied with a writhing in his body, as if meant to force an eſcape: Beſides theſe two ſhocks from the ſurface of the water, which may yet be conſidered as delivered in the air, he conſtantly gave at leaſt two, when wholly in the air, and conſtantly one and ſometimes two, when wholly in the water. The ſhocks in water appeared, as far as ſenſation could decide, not to have near a fourth of the force of thoſe at the ſurface of the water, nor much more than a fourth of thoſe intirely in air.

The ſhocks received in a certain time were not, on this occaſion, counted by a watch, as they had been on a former, when fifty were delivered, in a minute and a half, by the animal in an inſulated and unagitated ſtate: But from the quickneſs, with which the immerſions were made, it may be preſumed there were full twenty of theſe in a minute; from whence the number of ſhocks, in that time, muſt have amounted to above an hundred. This experiment, therefore, while it diſcovered the comparative force between a ſhock in water and one in air, and between a ſhock delivered with greater exertion on the part of the animal and one with leſs, ſeemed to determine, that the charge of his organs with electricity was effected in an inſtant, as well as the diſcharge.

The Torpedo was then put into a flat baſket, open at top, but ſecured by a net with wide meſhes, and, in this confinement, was let down into the water, about a foot below the ſurface; being

being there touched, through the meshes, with only a single finger, on one of his electric organs, while the other hand was held, at a distance, in the water, he gave shocks, which were distinctly felt in both hands.

The circuit for the passage of the effect being contracted to the finger and thumb of one hand, applied above and below to a single organ, produced a shock, to our sensation, of twice the force of that in the larger circuit by the arms.

The Torpedo, still confined in the basket, being raised to within three inches of the surface of the water, was there touched with a short iron bolt, which was held, half above and half in the water, by one hand, while the other hand was dipped, as before, at a distance in the water; and strong shocks, felt in both hands, were thus obtained through the iron.

A wet hempen cord being fastened to the iron bolt, was held in the hand above water, while the bolt touched the Torpedo; and shocks were obtained through both those substances.

A less powerful Torpedo, suspended in a small net, being frequently dipped into water and raised again, gave, from the surface of the water, slight shocks through the net to the person holding it.

These experiments in water manifested, That bodies, immersed in that element, might be affected by immediate contact with the Torpedo; That the shorter the circuit in which the electricity

tricity moved, the greater would be the effect; And that the ſhock was communicable, from the animal in water, to perſons in air, through ſome ſubſtances.

How far harpoons and nets, conſiſting of wood and hemp, could in like circumſtances, as it has been frequently aſſerted, convey the effect, was not ſo particularly tried as to enable us to confirm it. I mention the omiſſion in the hope that ſome one may be induced to determine the point by expreſs trial.

We convinced ourſelves, on former occaſions, that the accurate Kæmpfer *, who ſo well deſcribes the effect of the Torpedo, and happily compares it with lightning, was deceived in the circumſtance, that it could be avoided by holding in the breath, which we found no more to prevent the ſhock of the Torpedo, when he was diſpoſed to give it, than it would prevent the ſhock of the Leyden Phial.

Several perſons, forming as many diſtinct circuits, can be affected by one ſtroke of the animal, as well as when joined in a ſingle circuit. For inſtance, four perſons, touching ſeparately his upper and lower ſurfaces, were all affected; two perſons likewiſe, after the electricity had paſſed through a wire into a baſin of water, tranſmitted it from thence, in two diſtinct channels, as their ſenſation convinced them, into another baſin of water, from whence it was conducted, probably in an united ſtate, by a ſingle wire. How

* Kæmpf. Amœn. Exot. 1712, p. 514.

much

much further the effect might be thus divided and subdivided into different channels, was not determined; but it was found to be proportionably weakened by multiplying these circuits, as it had been by extending the single circuit.

Something may be expected to be said of the parts of the animal immediately concerned in producing the electrical effect. The engraving, which accompanies this letter, while it shews the general figure of the Torpedo, gives an internal view of his electric organs. The Society will, besides, have a full anatomical description of these parts from the ingenious Mr. John Hunter, in a paper he has expresly written on the subject at my request. It would therefore be superfluous for me to say any thing either in regard to their situation or structure.

I have to observe, however, That in these double organs resides the electricity of the Torpedo, the rest of his body appearing to be no otherwise concerned in his electrical effect than as conducting it: That they are subject to the will of the animal; but whether, like other double parts so controlable, they are exercised, at times, singly as well as in concert, is difficult to be ascertained by experiment: That their upper and under surfaces are capable, from a state of equilibrium with respect to electricity, of being instantly thrown, by a meer energy, into an opposition of a *plus* and *minus* state, like that of the charged Phial: That, when they are thus charged, the upper surfaces of the two are in the same state of electricity; as are the under

4 surfaces

ſurfaces of the two, though in a contrary to that of the upper; for no ſhock can be obtained by an inſulated perſon touching both organs above, or both below: And that the production of the effect depends ſolely on an intercourſe being made between the oppoſite ſurfaces of the organs, whether taken ſingly or jointly.

All the parts bordering on the organs act, more or leſs, as conductors, either through their ſubſtance or by their ſuperficies. While an inſulated perſon, placing two fingers on the ſame ſurface of one or both organs, cannot be affected; if he removes one of his fingers to any ſuch contiguous part, he will be liable to a ſhock: but this ſhock will not be near, perhaps not half, ſo violent, as one taken immediately between the oppoſite ſurfaces of the organ; which ſhews the conduction to be very imperfect.

The parts, which conduct the beſt, are the two great lateral fins bounding the organs outwardly, and the ſpace lying between the two organs inwardly. All below the double tranſverſe cartilages ſcarcely conduct at all, unleſs when the fiſh is juſt taken out of water and is ſtill wet, the mucus, with which he is lubricated, ſhewing itſelf, as it dries, to be of an inſulating nature.

The organs themſelves, when uncharged, appeared to be, not interiorly we might ſuppoſe, but rather exteriorly, conductors of a ſhock. An inſulated perſon touching two Torpedos, lying near one another on a damp table, with fingers placed, one on the organ of one fiſh, and another on

on the organ of the other, was ſenſible of ſhocks, ſometimes delivered by one fiſh, and ſometimes by the other, as might be diſcovered by the reſpective winking of their eyes. That the organs, uncharged, ſerved ſome way or other as conductors, was confirmed with artificial electricity, in paſſing ſhocks by them; and in taking ſparks from them, when electrified.

The electric effect was never perceived by us to be attended with any motion or alteration in the organs themſelves, but was frequently accompanied with a little tranſient agitation along the cartilages which ſurround both organs: this is not diſcernible in the plump and turgid ſtate of the animal, while he is freſh and vigorous; but as his force decays, from the relaxation of his muſcles, his cartilages appear through the ſkin, and then the ſlight action along them is diſcovered.

May we not from all theſe premiſes conclude, that the effect of the Torpedo proceeds from a modification of the electric fluid? The Torpedo reſembles the charged Phial in that characteriſtic point of a reciprocation between its two ſurfaces. Their effects are tranſmitted by the ſame mediums; than which there is not perhaps a ſurer criterion to determine the identity of ſubtile matter: They, beſides, occaſion the ſame impreſſion on our nerves. Like effects have like cauſes. But it may be objected, that the effects of the Torpedo, and of the charged Phial, are not ſimilar in all their circumſtances; that the charged Phial occaſions attractive or repulſive diſpoſitions

in

in neighbouring bodies; and that its diſcharge is obtained through a portion of air, and is accompanied with light and ſound; nothing of which occurs with reſpect to the Torpedo.

The inaction of the electricity of the animal in theſe particulars, whilſt its elaſtic force is ſo great as to tranſmit the effect through an extenſive circuit and in its courſe to communicate a ſhock, may be a new phænomenon, but is no ways repugnant to the laws of electricity; for here too, the operations of the animal may be imitated by art.

The ſame quantity of electric matter, according as it is uſed in a denſe or rare ſtate, will produce the different conſequences. For example, a ſmall Phial, whoſe coated ſurface meaſures only ſix ſquare inches, will, on being highly charged, contain a denſe electricity capable of forcing a paſſage through an inch of air, and afford the phænomena of light, ſound, attraction, and repulſion. But if the quantity condenſed in this Phial, be made rare by communicating it to three large connected jars, whoſe coated ſurfaces ſhall form together an area 400 times larger than that of the Phial (I inſtance theſe jars becauſe they are ſuch as I uſe); it will, thus dilated, yield all the negative phænomena, if I may ſo call them, of the Torpedo; it will not now paſs the hundredth part of that inch of air, which in its condenſed ſtate it ſprung through with eaſe; it will now refuſe the minute interſection in the ſtrip of tinfoil; the ſpark and its attendant ſound, even the attraction or repulſion of light bodies, will now

 be

be wanting; nor will a point brought however near, if not in contact, be able to draw off the charge: and yet, with this diminished elasticity, the electric matter will, to effect its equilibrium, instantly run through a considerable circuit of different conductors, perfectly continuous, and make us sensible of an impulse in its passage.

Let me here remark, that the sagacity of Mr. Cavendish in devising and his address in executing electrical experiments, led him the first to experience with artificial electricity, that a shock could be received from a charge which was unable to force a passage through the least space of air.

But, after the discovery that a large area of rare electricity would imitate the effect of the Torpedo, it may be inquired, where is this large area to be found in the animal? We here approach to that veil of nature, which man cannot remove. This, however, we know, that from infinite division of parts infinite surface may arise, and even our gross optics tell us, that those singular organs, so often mentioned, consist like our electric batteries of many vessels, call them cylinders or hexagonal prisms, whose superficies taken together furnish a considerable area.

I rejoice in addressing these communications to You. He, who predicted and shewed that electricity wings the formidable bolt of the atmosphere, will hear with attention, that in the deep it speeds an humbler bolt, silent and invisible: He, who analysed the electrified Phial, will hear with

pleaſure that its laws prevail in animate Phials: He, who by Reaſon became an electrician, will hear with reverence of an inſtinctive electrician, gifted in his birth with a wonderful apparatus, and with the ſkill to uſe it.

However I may reſpect your talents as an electrician, it is certainly for knowledge of more general import, that I am impreſſed with that high eſteem, with which I remain,

Dear Sir,

Your affectionate

and obedient ſervant,

John Walſh.

E X.

EXPLANATION of the PLATE

OF THE

Male and Female Torpedo, or Electric Ray.

T A B. XIX.

F I G. I.

A view of the under ſurface of the female.

a. An expoſure, on flaying off the ſkin, of the right electric organ, which conſiſts of white pliant columns, in a cloſe and for the moſt part hexagonal arrangement, giving the general appearance of a honey-comb in miniature. Theſe columns have been ſometimes denominated cylinders; but, having no interſtices, they are all angular, and chiefly ſix-cornered.

b. The ſkin which covered the organ, ſhewing on its inward ſide an hexagonal net work.

c. The noſtrils in the form of a creſcent.

d. The mouth in a creſcent contrary to that of the noſtrils, furniſhed with ſeveral rows of very ſmall hooked teeth.

e. The

e. The branchial apertures, five on each ſide.

f. The place of the heart.

g. g. g. The place of the two anterior tranſverſe cartilages, which, paſſing one above and the other below the ſpine, ſupport the diaphragm, and uniting towards their extremities, form on either ſide a kind of clavicle and ſcapula.

h. h. The outward margin of the great lateral fin.

i. i. Its inner margin, confining with the electric organ.

k. The articulation of the great lateral fin with the ſcapula.

l. The abdomen.

m. m. m. The place of the poſterior tranſverſe cartilage which is ſingle, united with the ſpine, and ſupports on each ſide the ſmaller lateral fins.

n. n. } The two ſmaller lateral fins.
n. n. }

o. The anus.

p. The fin of the tail.

FIG. II.

A view of the upper ſurface of the female.

a. a. An expoſure of the upper part of the right electric organ.
b. The ſkin which covered the organ.
c. The eyes, prominent and looking horizontally outwards, but capable of being occaſionally withdrawn into their ſockets.
d. Two circular apertures communicating with the mouth, and furniſhed each with a membrane, which in air, as well as in water, plays regularly backwards and forwards acroſs the aperture in the office of inſpiration.
e. The place of the right branchia.
f. The two fins of the back.
g. g. The place of the anterior tranſverſe cartilages.

FIG. III.

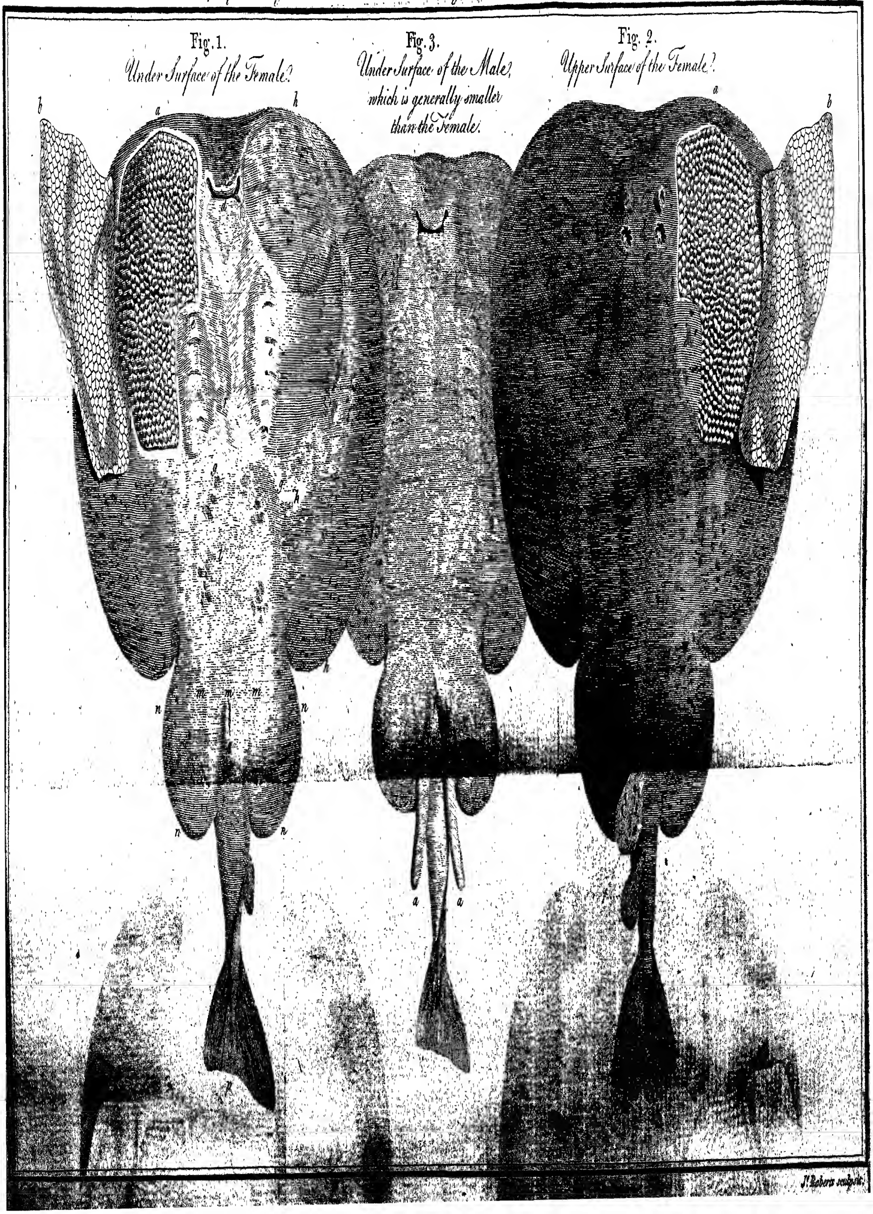
A view of the under ſurface of the male, whoſe ſize, as here repreſented, is, in general, ſmaller than that of the female.

a. a. Two appendices, diſtinguiſhing the male ſpecies.

The Male & Female TORPEDO or ELECTRIC RAY,

frequenting the Sea Shores in the Neighbourhood of LA ROCHELLE.

Philos Trans Vol. LXIII. Tab. XIX p. 480.



J. Roberts sculpsit.

XL. *Anatomical Obſervations on the Torpedo.* *By* John Hunter, *F. R. S.*

Read July 1, 1773.

I WAS deſired ſome time ſince, by Mr. Walſh, whoſe experiments at La Rochelle had determined the effect of the Torpedo to be electrical, to diſſect and examine the peculiar organs by which that animal produces ſo extraordinary an effect. This I have done in ſeveral ſubjects furniſhed to me by that Gentleman.

I am now deſired by him to lay before the Society, the obſervations I have made; and for the better underſtanding of them, to preſent, on his part, a male and female Torpedo in ſpirits; in the latter of which the electric organs are expoſed in different views and ſections; likewiſe a copper-plate, which he took care to have engraved, exhibiting thoſe organs.

Of the general ſtructure and anatomy of the Torpedo I ſay nothing, ſince the animal does not differ very materially, excepting in it's electric organs (as they have been properly named by Mr. Walſh) from the reſt of the Rays, of which family it is well known to be. I will only premiſe, that the Torpedo, of which I treat, is about eighteen inches long, twelve broad, and in it's central or thickeſt part two inches thick; which is nearly the ſize of the

the female ſpecimen, now preſented to the Society, as well as of that from which the plate was taken: but where there is any difference in the organ ariſing from difference in ſize, notice will be taken of it in this account.

The electric organs of the Torpedo are placed on each ſide of the *cranium* and gills, reaching from thence to the ſemicircular cartilages of each great fin, and extending longitudinally from the anterior extremity of the animal to the tranſverſe cartilage, which divides the thorax from the abdomen; and within theſe limits they occupy the whole ſpace between the ſkin of the upper and of the under ſurfaces: they are thickeſt at the edges near the center of the fiſh, and become gradually thinner towards the extremities. Each electric organ, at it's inner longitudinal edge, is unequally hollowed; being exactly fitted to the irregular projections of the *cranium* and gills. The outer longitudinal edge is a convex elliptic curve. The anterior extremity of each organ, makes the ſection of a ſmall circle; and the poſterior extremity makes nearly a right angle with the inner edge. Each organ is attached to the ſurrounding parts by a cloſe cellular membrane, and alſo by ſhort and ſtrong tendinous fibres, which paſs directly acroſs, from it's outer edge, to the ſemicircular cartilages.

They are covered, above and below, by the common ſkin of the animal; under which there is a thin *faſcia* ſpread over the whole organ. This is compoſed of fibres, which run longitudinally, or in the direction of the body of the animal: theſe fibres appear to be perforated in innumerable places; which

gives the *faſcia* the appearance of being faſciculated; it's edges all around, are cloſely connected to the ſkin, and at laſt appear to be loſt, or to degenerate into the common cellular membrane of the ſkin.

Immediately under this, is another membrane, exactly of the ſame kind, the fibres of which in ſome meaſure decuſſate thoſe of the former, paſſing from the middle line of the body outwards and backwards. The inner edge of this, is loſt with the firſt deſcribed; the anterior, outer, and poſterior edges are partly attached to the ſemicircular cartilages, and partly loſt in the common cellular membrane."

This inner *faſcia* appears to be continued into the electric organ, by ſo many proceſſes, and thereby makes the membranous ſides or ſheaths of the columns, which are preſently to be deſcribed; and between theſe proceſſes the *faſcia* covers the end of each column, making the outermoſt or firſt partition.

Each organ, of the fiſh under conſideration, is about five inches in length, and at the anterior end three in breadth, though it is but little more than half as broad at the poſterior extremity.

Each conſiſts wholly of perpendicular columns, reaching from the upper to the under ſurface of the body, and varying in their lengths, according to the thickneſs of the parts of the body where they are placed; the longeſt column being about an inch and an half, the ſhorteſt about one fourth of an inch in length, and their diameters about two tenths of an inch.

The

The figures of the columns are very irregular, varying according to ſituation and other circumſtances. The greateſt number of them are either irregular Hexagons, or irregular Pentagons; but from the irregularity of ſome of them, it happens that a pretty regular quadrangular column is ſometimes formed. Thoſe of the exterior row are either quadrangular or hexagonal; having one ſide external, two lateral, and either one or two internal. In the ſecond row they are moſtly pentagons.

Their coats are very thin, and ſeem tranſparent, cloſely connected with each other, having a kind of looſe network of tendinous fibres, paſſing tranſverſely and obliquely between the columns, and uniting them more firmly together. Theſe are moſtly obſervable where the large trunks of the nerves paſs. The columns are alſo attached by ſtrong inelaſtic fibres, paſſing directly from the one to the other.

The number of columns in different Torpedos of the ſize of that now offered to the Society, appeared to be about 470 in each organ, but the number varies according to the ſize of the fiſh *. Theſe columns increaſe, not only in ſize, but in number, during the growth of the animal: new ones forming perhaps every year on the exterior edges, as there they are much the ſmalleſt. This proceſs may be ſimilar to the formation of new teeth, in the human jaw, as it increaſes.

* In a very large Torpedo, the number of columns in one electric organ were 1182.

Each column is divided by horizontal partitions, placed over each other, at very ſmall diſtances, and forming numerous interſtices, which appear to contain a fluid. Theſe partitions conſiſt of a very thin membrane, conſiderably tranſparent. Their edges appear to be attached to one another, and the whole is attached by a fine cellular membrane to the inſide of the columns. They are not totally detached from one another: I have found them adhering, at different places, by blood-veſſels paſſing from one to another.

The number of partitions contained in a column of one inch in length, of a Torpedo which had been preſerved in proof ſpirit, appeared upon a careful examination to be one hundred and fifty: and this number in a given length of column appears to be common to all ſizes in the ſame ſtate of humidity, for by drying they may be greatly altered; whence it appears probable that the increaſe in the length of a column, during the growth of the animal, does not enlarge the diſtance between each partition in proportion to that growth; but that new partitions are formed, and added to the extremity of the column from the *faſcia*.

The partitions are very vaſcular; the arteries are branches from the veins of the gills, which convey the blood that has received the influence of reſpiration. They paſs along with the nerves to the electric organ, and enter with them; then they ramify, in every direction, into innumerable ſmall branches upon the ſides of the columns, ſending in from the circumference all around upon each partition ſmall

arteries,

arteries, which ramify and anaſtomoſe upon it; and paſſing alſo from one partition to another, anaſtomoſe with the veſſels of the adjacent partitions.

The veins of the electric organ paſs out, cloſe to the nerves, and run between the gills, to the auricle of the heart.

The nerves inſerted into each electric organ, ariſe by three very large trunks from the lateral and poſterior part of the brain. The firſt of theſe, in it's paſſage outwards, turns round a cartilage of the *cranium*, and ſends a few branches to the firſt gill, and to the anterior part of the head, and then paſſes into the organ towards its anterior extremity. The ſecond trunk enters the gills between the firſt and ſecond openings, and, after furniſhing it with ſmall branches, paſſes into the organ near it's middle. The third trunk, after leaving the ſkull, divides itſelf into two branches, which paſs to the electric organ through the gills; one between the ſecond and third openings, the other between the third and fourth, giving ſmall branches to the gill itſelf. Theſe nerves having entered the organs, ramify in every direction, between the columns, and ſend in ſmall branches upon each partition where they are loſt.

The magnitude and the number of the nerves beſtowed on theſe organs, in proportion to their ſize, muſt on reflection appear as extraordinary as the phænomena they afford. Nerves are given to parts either for ſenſation or action. Now if we except the more important ſenſes of ſeeing, hearing, ſmelling, and taſting, which do not belong to the electric organs, there is no part even of the moſt perfect animal, which, in proportion to it's ſize, is

ſo

so liberally supplied with nerves; nor do the nerves seem necessary for any sensation which can be supposed to belong to the electric organs. And with respect to action, there is no part of any animal, with which I am acquainted, however strong and constant it's natural actions may be, which has so great a proportion of nerves.

If it be then probable, that those nerves are not necessary for the purposes of sensation, or action, may we not conclude that they are subservient to the formation, collection, or management of the electric fluid; especially as it appears evident, from Mr. Walsh's experiments, that the will of the animal does absolutely controul the electric powers of it's body; which must depend on the energy of the nerves.

How far this may be connected with the power of the nerves in general, or how far it may lead to an explanation of their operations, time and future discoveries alone can fully determine.

An EXPLANATION of the Engraving of the TORPEDO.

TAB. XX.

Fig. I. The upper ſurface of the electric organ.

AA. The common ſkin of the animal.

B. The inſpiratory opening.

C. The eye.

D. The part in which the gills are incloſed.

EEE. The ſkin diſſected off from the electric organ, and turned outwards; the honeycomb appearance on it's internal ſurface correſponding with the upper ſurface of the organ.

F. The part of the ſkin which covered the gills, with ſome ramifications of an excretory duct upon it.

GGG. The upper ſurface of the electric organ, formed by the upper extremities of the perpendicular columns.

Fig. II. The right electric organ divided horizontally into nearly two equal parts at the place where the nerves enter; the upper half being turned outwards.

AA. BB. CC. DD. The correſponding parts of the trunks of the nerves, as they emerge from the gills, and ramify in the electric organ.

AA. The firſt or anterior trunk ariſing juſt before the gills.

BB. The ſecond or middle trunk ariſing behind the firſt gill.

E. The

CC. The anterior branch of the third trunk arifing behind the fecond gill.

DD. The pofterior branch of the third trunk arifing behidd the third gill.

Fig. III. A perpendicular fection of the Torpedo a little below its infpiratory openings.

AA. The upper furface of the fifh.

BB. The mufcles of the back as divided by the fection.

C. The Medulla Spinalis.

D. The Oefophagus.

E. The left gill fplit, to expofe the courfe of a trunk of the nerve through it.

F. The breathing furface of the right gill.

GG. The fins.

HH. The perpendicular columns which compofe the electric organ, with a reprefentation of their horizontal partitions.

I. One of the trunks of the nerves, with its ramifications.

Fig.

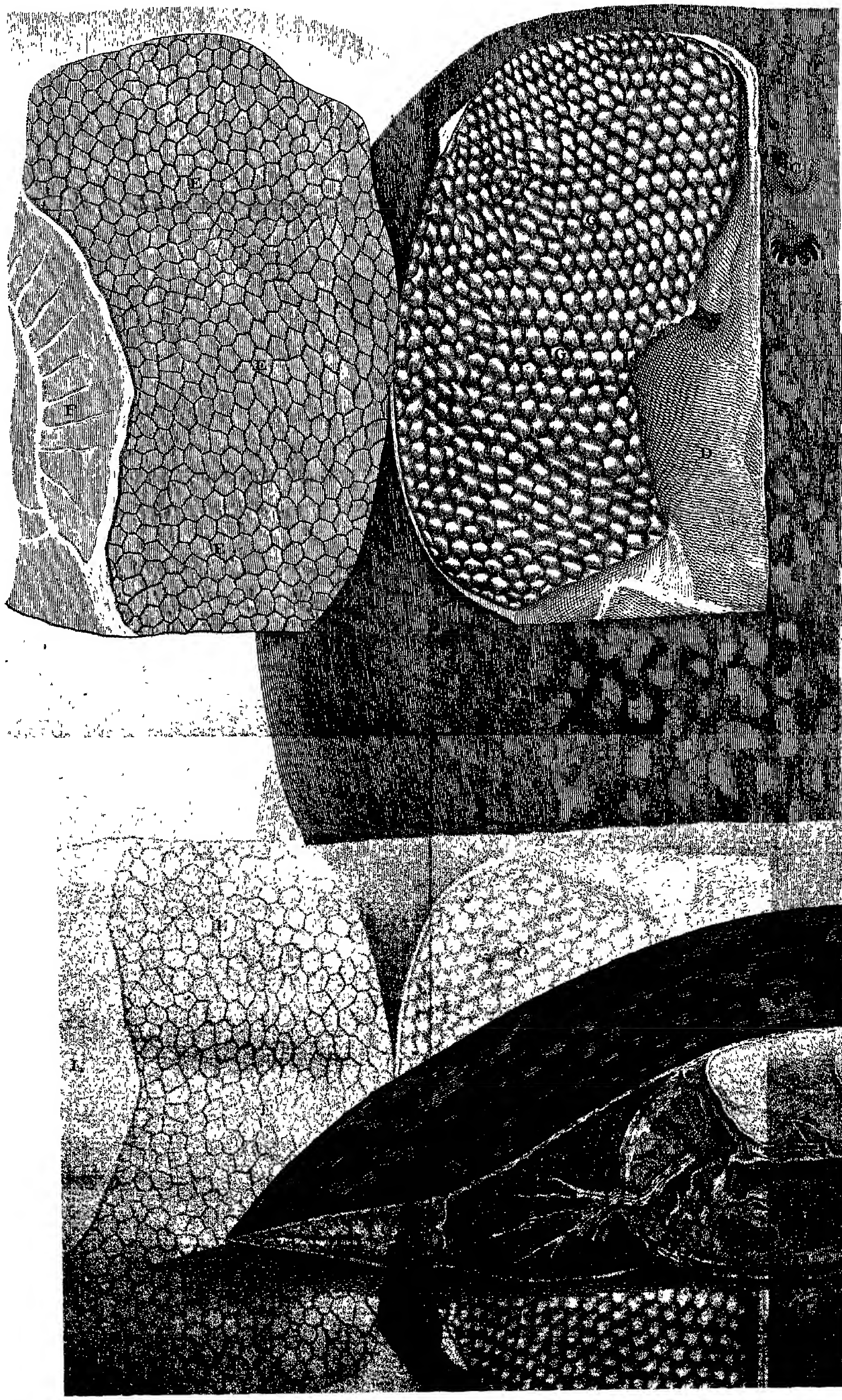


Fig 2

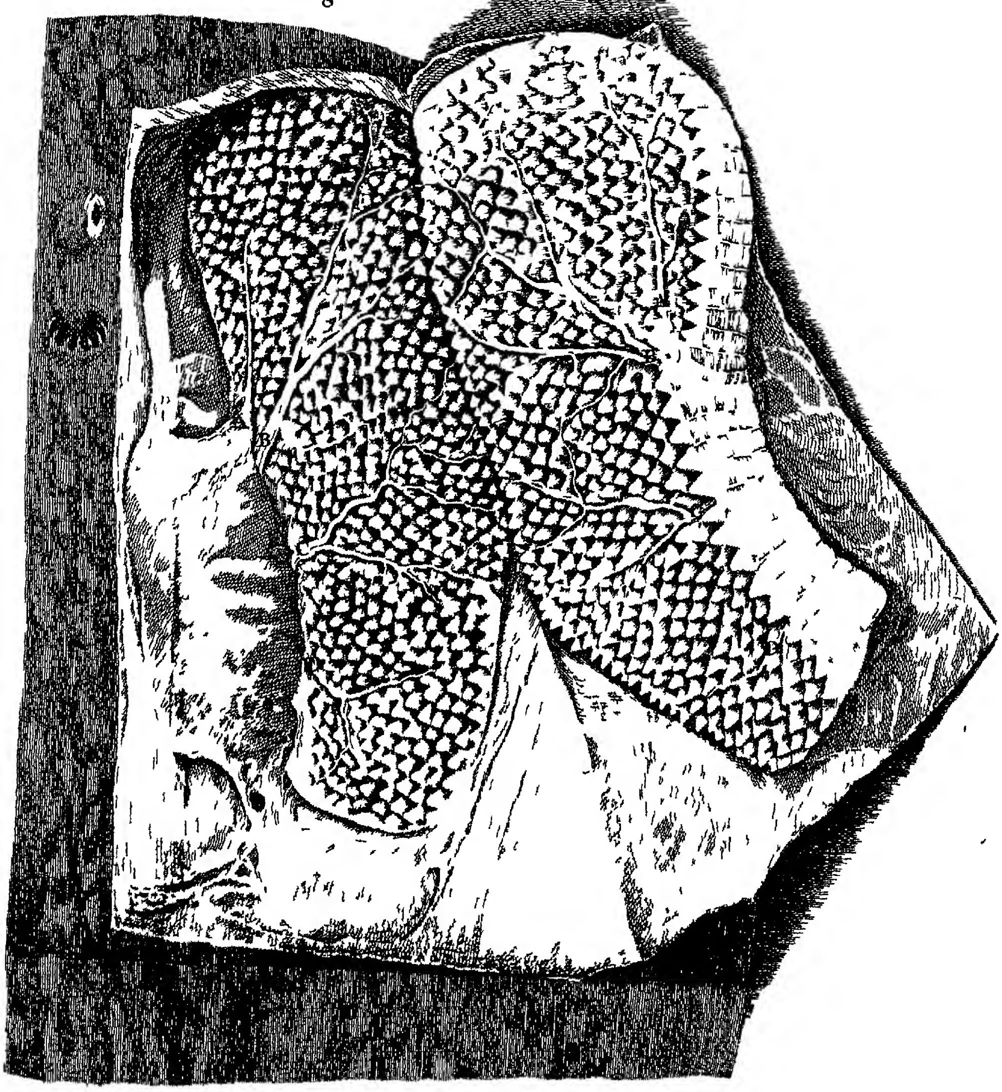
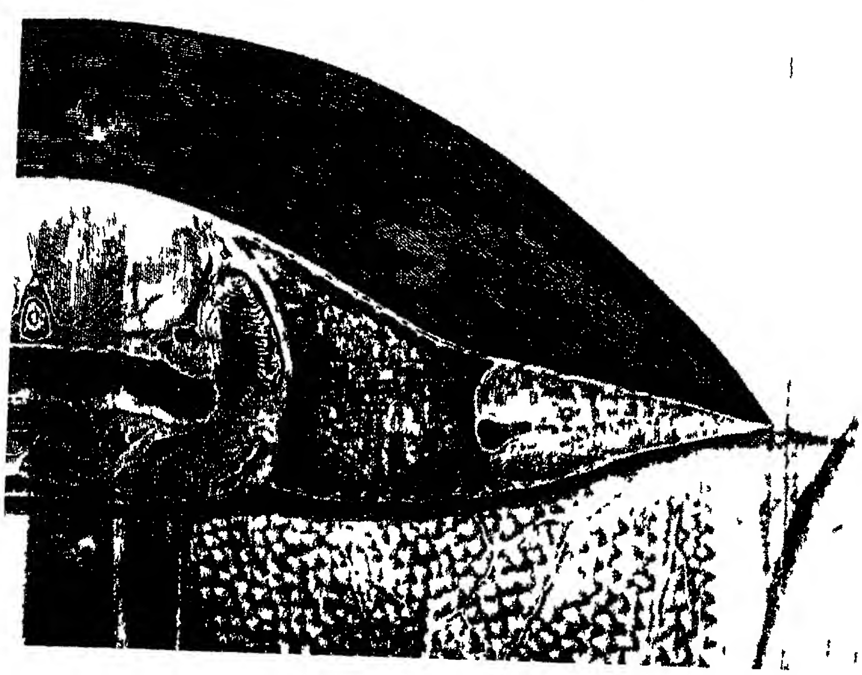


Fig. 3



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